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## **Families and Schools Together (FAST) for improving outcomes for children and their families (Review)**

Valentine JC, Leach SM, Fowler AP, Stojda DK, Macdonald G

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## [Intervention Review]

# Families and Schools Together (FAST) for improving outcomes for children and their families

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## ABSTRACT

### Background

Parents and carers have a major influence on children's learning and development from birth, through the school years, and into adulthood. Parental contributions to education include providing a secure environment in which to learn, providing intellectual stimulation, transmitting social norms and values, shaping the child's resilience through fostering literacy and problem-solving, and encouraging personal and social aspiration. Increasingly, providers of formalised education are recognising the primary role of parents, carers, and the wider family, as well as peers and the environment, in shaping children's education, health, and life experiences.

### Objectives

To assess the effectiveness of the Families and Schools Together (FAST) programme in improving outcomes among children and their families.

### Search methods

Between October 2018 and December 2018, we searched CENTRAL, MEDLINE, Embase, PsycINFO, 11 additional databases, and three trial registers. We handsearched the reference lists of included studies and relevant reports and reviews, contacted the programme developer and independent researchers, and searched relevant websites to identify other eligible studies.

### Selection criteria

We included randomised controlled trials (RCTs) and quasi-RCTs examining the effects of FAST, relative to waiting list, usual or alternative services, or no intervention, on outcomes for children (aged from birth to completion of compulsory education) and their families.

### Data collection and analysis

At least two review authors independently evaluated the records retrieved from the search for relevance. One review author (JV) extracted data from eligible studies with a second independent review author (AF, DK, or SL). Review authors consulted with one another to resolve disagreements. We used a fixed-effect model for meta-analysis. We presented results as standardised mean differences (SMDs) because all outcomes were continuously scaled, and we accompanied these with 95% confidence intervals (CIs). We used the GRADE approach to assess the certainty of evidence for each outcome.

## Main results

We identified 10 completed RCTs, most of which were relatively recent (2007 or later) and were conducted with at least some involvement from the intervention developer or the FAST organisation. Nine of the 10 trials were from the USA; the other was from the UK. Children were young (five to nine years old; mean age approximately six years), and therefore, whilst not so named in the reports, evaluations consisted of what is sometimes referred to as 'Kids FAST' and sometimes 'Elementary Level FAST'. Among the USA-based studies, at least 62% of participants were members of a racial/ethnic minority group (most commonly, African American or Latino). FAST was usually delivered in schools after the school day. Trials lasted about eight weeks and usually examined the effects of FAST relative to no additional intervention. Most studies were funded by agencies in the US federal government. We judged the certainty of evidence in the included studies to be moderate or low for the main review outcomes. Failure to include all families in outcome analyses (attrition) and possible bias in recruitment of families into the trials were the main limitations in the evidence.

We included over 9000 children and their families in at least one meta-analysis. The follow results relate to meta-analyses of data at long-term follow-up.

## Primary outcomes

Four studies (approximately 6276 children) assessed child school performance at long-term follow-up. The effect size was very small, and the CI did not include effects that, if real, suggest possibly important positive or negative effects if viewed from an individual perspective (SMD -0.02, 95% CI -0.11 to 0.08). We assessed the certainty of evidence for this outcome as moderate. No studies assessed child adverse events, parental substance use, or parental stress.

## Secondary outcomes

Parent reports of child internalising behaviour (SMD -0.03, 95% CI -0.11 to 0.17; 4 RCTs, approximately 908 children; low-certainty evidence) and family relationships (SMD 0.08, 95% CI -0.03 to 0.19; 4 RCTs, approximately 2569 children; moderate-certainty evidence) also yielded CIs that did not include effects that, if real, suggest possibly important positive or negative effects.

The CI for parent reports of child externalising behaviour, however, did include effects that, if real, were possibly large enough to be important (SMD -0.19, 95% CI -0.32 to -0.05; 4 RCTs, approximately 754 children; low-certainty evidence).

## Authors' conclusions

Given these results, it is hard to support the assertion that assignment to FAST is associated with important positive outcomes for children and their parents.

## PLAIN LANGUAGE SUMMARY

### Families and Schools Together

#### Background

Children who do well at school have carers who are interested in their learning and who encourage them to read and solve problems and do the best they can. Carers who live in supportive communities often find it easier to provide this kind of parenting than those who are more isolated. Families and Schools Together - known as FAST - is a programme that is designed to help parents help their children to do well at school. It aims to address the problems that prevent parents from giving their children the support they need, by improving relationships between families and schools, enhancing support for families, and tackling problems such as poverty, mental illness, and substance misuse. Five different versions of FAST have been developed for families of children at different ages.

#### Review question

Does the Families and Schools Together (FAST) programme improve outcomes for children and their families?

#### Study characteristics

We found 10 randomised controlled studies (studies where schools were assigned to receive FAST or to continue as usual, by a procedure similar to tossing a coin), with a total of more than 9000 children and their families.

Nine of the studies took place in the USA and were funded by agencies in the US federal government. One study took place in the UK. Children's ages ranged from five to nine years, and most of the USA-based children were members of a racial or ethnic minority group. Boys and girls were represented at approximately equal rates. In most studies, FAST was delivered at children's schools after the end of the school day, although in some studies it was delivered outside of school (e.g. at a community centre). The trials lasted about eight weeks and usually examined the effects of FAST compared to no additional intervention. The evidence is current as of December 2018.

## Results

A meta-analysis is a statistical method of combining data from several studies to reach a single, more robust conclusion. We were able to use data from nine studies in a meta-analysis measuring the impact of FAST for children aged between five and eight years. Although individual studies reported some positive findings, there was little evidence to suggest that being involved in a FAST programme results in important improvements in the primary outcomes of child school performance, parental substance abuse, or parental stress. No study measured child adverse outcomes. Furthermore, there was little evidence to suggest that FAST leads to important improvements in child behaviour or family relations.

**Study quality**

We judged the certainty of evidence in the included studies for the main review outcomes to be moderate or low risk. Failure to include all families in outcome analyses (attrition) and possible bias in recruitment of families into the trials were the main limitations in the evidence.

**Authors' conclusions**

Evidence on the effectiveness of being assigned to FAST is of moderate to low certainty and does not suggest that being assigned to FAST confers important benefits for students and their families.

## SUMMARY OF FINDINGS

**Summary of findings for the main comparison. Families and Schools Together (FAST) compared to no intervention (or care as usual) for children and their families**

### Families and Schools Together (FAST) compared to no intervention (or care as usual) for children and their families

**Patient or population:** children and their families  
**Setting:** schools and community centres  
**Intervention:** Families and Schools Together (FAST)  
**Comparison:** no intervention (or care as usual)

Outcomes	Anticipated absolute effects* (95% CI)		Relative effect (95% CI)	Nº of participants (studies)	Certainty of the evidence (GRADE)	Comments
	Risk with no intervention (or care as usual)	Risk with FAST				
<b>Child school performance: long-term follow up</b> Assessed with standardised test scores in math and reading Follow-up: range 9 months to 12 months	-	Mean score in the intervention group was <b>0.02 standard deviations lower</b> (0.11 lower to 0.08 higher)	-	6276 children (approximately) (4 RCTs)	⊕⊕⊕⊖ <b>Moderate<sup>a</sup></b>	Positive effect sizes indicate improvement. Evidence suggests that FAST probably does not have an important positive effect on child school performance at long-term follow-up
<b>Child adverse events</b> - not measured	-	-	-	-	-	-
<b>Parental substance abuse</b> - not measured	-	-	-	-	-	-
<b>Parental stress</b> - not measured	-	-	-	-	-	-
<b>Child internalising behaviours</b> Assessed with parent reports of multiple scales designed to assess internalising symptoms Follow-up: range 9 months to 12 months	-	Mean score in the intervention group was <b>0.03 standard deviations lower</b> (0.11 lower to 0.17 higher)	-	908 (approximately) (4 RCTs)	⊕⊕⊖⊖ <b>Low<sup>b,c,d</sup></b>	Negative effect sizes indicate improvement. Evidence suggests that FAST probably does

					not result in an important reduction in child internalising behaviours	
<b>Child externalising behaviours</b> Assessed with parent reports of multiple scales designed to assess externalising symptoms Follow-up: range 9 months to 12 months	-	Mean score in the intervention group was <b>0.19 standard deviations lower</b> (0.32 lower to 0.05 lower)	-	754 (approximately)  (4 RCTs)	⊕⊕⊕⊕ <b>Low</b> <sup>b,d</sup>	Negative effect sizes indicate improvement. FAST may result in a small and possibly important positive effect on parent reports of child externalising behaviour at follow-up. However, teacher reports for this outcome suggest small and probably unimportant effect sizes, as did parent reports of child externalising behaviours immediately after completion of the intervention
<b>Family relationships</b> Assessed with parent reports of multiple scales designed to assess family relationships Follow-up: range 9 months to 12 months	-	Mean score in the intervention group was <b>0.08 standard deviations higher</b> (0.03 lower to 0.19 higher)	-	2569 (approximately)  (4 RCTs)	⊕⊕⊕⊕ <b>Moderate</b> <sup>e</sup>	Positive effect sizes indicate improvement. FAST may result in a small and probably unimportant positive effect on parent reports of family relations

\*The risk in the intervention group (and its 95% CI) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).



CI: confidence interval; FAST: Families and Schools Together; RCT: randomised controlled trial.

---

**GRADE Working Group grades of evidence.**

**High certainty:** we are very confident that the true effect lies close to that of the estimate of the effect.

**Moderate certainty:** we are moderately confident in the effect estimate: the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different.

**Low certainty:** our confidence in the effect estimate is limited: the true effect may be substantially different from the estimate of the effect.

**Very low certainty:** we have very little confidence in the effect estimate: the true effect is likely to be substantially different from the estimate of effect.

---

<sup>a</sup>Certainty of evidence downgraded because two studies were at high risk for recruitment bias and two studies were at high risk for incomplete outcome data (attrition bias).

<sup>b</sup>Certainty of evidence downgraded due to indirectness because measurement is based on a single informant (parent) who does not have good access to child behaviour in other contexts (e.g. at school).

<sup>c</sup>Certainty of evidence downgraded due to imprecision (95% confidence interval suggests effect sizes indicating both potentially meaningful beneficial effects and potentially meaningful harmful effects).

<sup>d</sup>Certainty of evidence downgraded because all four studies included in this meta-analysis were at risk for incomplete outcome data (attrition bias) and three of the four studies were at high risk for recruitment bias.

<sup>e</sup>Certainty of evidence downgraded because three of the four studies included in this meta-analysis were at high risk for incomplete outcome data (attrition bias) and three of the four studies were at high risk for recruitment bias.

## BACKGROUND

### Description of the condition

The *Melbourne Declaration on Education Goals for Young Australians* is not unusual in recognising the important role played by schools in nurturing the "intellectual, physical, social, emotional, moral, spiritual, and aesthetic development and well-being" of young people (MCEETYA 2008). In the UK, the Education Act 2002 and the Academies Act 2010 require schools to provide a curriculum that promotes "the spiritual, moral, cultural and physical development of pupils at [...] school and of society, and prepare pupils ... for the opportunities, expendabilities and experiences of later life". Schools are not, therefore, *just* about educational attainment; they are also about how well children engage with school; in turn, how children do at school is predictive of a range of outcomes in later life, including future employment, income, and physical and mental health (see, for example, Heckmann 2014; Nores 2010).

Investment in education and training is essential for creating sustainable economic growth, competitiveness, and increased productivity (European Commission 2012b). One of the five flagship initiatives of Europe's Growth Strategy is to reduce school dropout rates to below 10% in member nations, where, on average, 73 million adults have low levels of education and 20% of 15-year-olds lack basic reading skills (European Commission 2013). The Education and Training Monitor supports efforts to combat early school leaving, to increase participation in education, to improve early education experiences, to reduce inequalities in achievements, and to promote skills-based learning and participation in education (European Commission 2012a). As part of the global drive to increase the uptake of education (ACARA 2013; Eurydice 2012; National Committee on Inuit Education 2011; USDoe 2002), many education systems have introduced some form of objective-based educational standards designed to record levels of achievement or 'outcomes' in basic skills such as literacy, numeracy, science, languages, and social skills (The World Bank 2009).

Children whose families live in poverty, particularly persistent poverty, are at increased risk of low educational achievement. The reasons for this are complex, reflecting the interaction of individual, family, school, and community level factors. Beyond the direct negative impacts of poverty (examples of which can include poor housing and nutrition), individual factors that have been indirectly implicated in children's educational attainment include a child's working memory (Alloway 2010); information-processing efficiency in infancy, general cognitive development in toddlerhood, and behaviour difficulties in early childhood (Bornstein 2013); and children's health (Basch 2011); family factors include mother's educational attainment (Bornstein 2013); school absences and mobility (Ou 2008); and parental aspirations (Goodman 2011). The quality of the school environment also influences outcomes for children (Sylva 2011), as do community factors such as the level of social disorganisation (Nieuwenhuis 2016).

The adverse impact of poverty makes itself felt in children's earliest years, with children from economically disadvantaged backgrounds performing well behind their non-disadvantaged peers in terms of literacy and communication skills, and school readiness by three years of age (Hirsh 2007). These differences persist and the gap between those in poverty and their more affluent peers increases over time. Good quality parenting can play an important role in mitigating the effects of poverty, and can help to ensure

that children reach their educational potential (Kiernan 2011). Sensitive, positive parenting can help ensure that children are ready for school, and can support their learning once there (e.g. Jeynes 2005). However, the stresses of poverty may adversely impact parents' ability to parent their children effectively, and this can be exacerbated by an absence of support networks (Gutman 2010).

Since publication of the Coleman Report (Coleman 1966), which asserted that schools have less influence upon student outcomes than family background and other environmental factors (Emerson 2012), policy makers have adopted increasingly broad-based approaches to improving educational outcomes, with increasing emphasis upon family involvement within a supported learning environment. Multi-systemic approaches, which involve staff, students, parents, and the wider community, are thought to have a greater chance of success in improving child outcomes.

Parents and carers have an important role in the socialisation of young people (Foxcroft 2011). They exert a major influence on children's learning and development from birth, through the school years, and long into adulthood (DEEWR 2011). Parental contributions to education include providing a secure environment in which to learn, providing intellectual stimulation, transmitting social norms and values, shaping the child's resilience through fostering literacy and problem-solving, and encouraging personal and social aspiration (Kim 2012). Increasingly, providers of formalised education are recognising the primary role of parents, carers, and the wider family, as well as peers and the environment, in shaping children's education, health, and life experiences (Desforges 2003; Sisco 2012). Research suggests that high levels of parental and community involvement in education are related to improved student performance, learning outcomes, attendance, and behaviour, regardless of cultural and social background (DEEWR 2011; Weiss 2010).

### Description of the intervention

FAST (Families and Schools Together) was developed in the USA in 1988 at Family Service Incorporated, in Madison, Wisconsin, by Dr Lynn McDonald, in collaboration with the Prevention - Intervention Centre for Alcohol and Other Drug Abuse. It was originally designed as a targeted intervention for children at risk of failure at school (Layzer 2001). Since that time, FAST has evolved into a multi-family, after-school programme, which is primarily implemented in schools with populations experiencing multiple risk factors of deprivation (Layzer 2001; McDonald 2009a; WSiPP 2012). The programme has incorporated cultural adaptations to suit White, Hispanic/Latino, American Indian, African American, Southeast Asian American, Alaskan Native, and Australian Indigenous people, and it has been translated into French, German, Japanese, Russian, and Spanish, and has been delivered to multi-lingual and English groups as a 'second language' (CBCAP 2009; Mupotsa 2010). FAST is in use in over 2000 schools across 11 countries, including the USA, Canada, Australia, Europe and Russia, Brazil, and the UK (McDonald 2010).

McDonald describes FAST as a programme based on family systems theory, and on evidence-based, published, and funded research on best practices. Overall, FAST is designed to prevent children from experiencing school failure by empowering parents in their role as educators, by fostering closer relationships between families and schools, and by encouraging improvements in children's behaviour and educational outcomes. FAST works with health profes-

sionals to prevent substance abuse through increasing knowledge and awareness of effects of substance abuse on child development and by generating links between the programme, local substance abuse, and mental health services. FAST also aims to reduce family stress by developing ongoing supports for parents and children, by facilitating parents' access to local supports and agencies, and by fostering personal achievement and self-esteem in participants. This element of the programme is referred to as FASTWORKS.

FAST is delivered in a number of age-specific versions, which have been categorised in various ways. The table below provides an overview of the core programmes, details of which can be found at [www.familiesandschools.org](http://www.familiesandschools.org). Details of Baby FAST can be found in [Fletcher 2013](#).

### FAST versions across age groups

FAST version	Age group
Baby FAST	For young parents and their infants from birth to 3 years of age
Elementary school level	For families and their children aged 3 to 10 years
Middle school level	For parents and students from 11 to 14 years of age
High school level	For families and students aged 13 to 18 years

### The FAST curriculum

The curriculum comprises a set of core elements, 40% of which must be implemented 'precisely' in each of the age-specific versions. The remaining 60% can be adapted as required to best meet the needs of participating families.

For each FAST version, there are two levels of implementation: 'standard' for a single site, and 'multi-hub' for multiple, "simultaneous cycles at the same location" ([www.familiesandschools.org/programs](http://www.familiesandschools.org/programs)). Furthermore, each version can be delivered in one of three editions, as listed below.

1. Parent Involvement FAST, with an emphasis on drug and alcohol misuse.
2. Healthy FAST, concentrating on physical and mental health.
3. Achieve FAST, for families of children with special needs.

In all its versions, participation is voluntary, with families invited to attend FAST sessions (usually held after school and, with the exception of Baby FAST, on school premises).

The core programme comprises 8 to 12 weekly sessions of 2.5 hours' duration, usually delivered to between 5 and 10 families by a school site team comprising a professional from the host school (teacher), a mental health professional from FAST, an Alcohol or Drugs Professional, and a parent liaison who has graduated from the FAST programme. In each session, parents are helped to lead their family in "sharing a meal, singing, doing family crafts and playing fun communication games for one hour" ([McDonald 2012a](#), p 79). In the following hour, families separate into parent-only and children-only play time. The parents first talk in pairs for 15 minutes ('buddy time'), then participate in a 45-minute shared session with other parents. This session has no set agenda and is designed to foster bonding and to help parents to 'find their voice'. In the last half hour, parents are coached in how to play responsively for 15 minutes with one child; this is followed by a whole family session organised around routine closing activities. This last ses-

sion includes a lottery in which every family wins once. The focus is on experiential learning rather than didactic teaching. This core programme encompasses families with children aged 3 to 13/14 years (eight grade), with activities tailored to the age groups being served.

A typical session consists of a family meal, family communication games, a parental self-help group session, supervised children's play, one-to-one mediated parental play therapy, opening and closing routines, and family rituals. Whilst 40% of the programme must be implemented precisely, the remaining 60% can be adapted to meet the needs of the target population.

At the end of the eight-week programme, families are encouraged to meet monthly over a two-year period under the acronym FASTWORKS (Families and Schools Together, Working, Organising, Relaxing, Knowing, Sharing). FASTWORKS is organised by a Parent Advisory Council that is made up of elected FAST 'graduates', each of whom is given a budget and responsibility to plan and implement the two-year programme.

### Baby FAST

Baby FAST is specifically designed for young mothers and their infants aged from birth to three years. It is designed to "engage young parents, particularly teenagers, in a socially inclusive experience, thereby counteracting the social disapproval and stigma they have encountered from professionals to date" ([Fletcher 2013](#), p 154). It also seeks to reduce the risks associated with social isolation, improve relationships between parents and grandparents, and reinforce protective factors. In Baby FAST, grandparents (or other support persons) are also involved. The programme structure is broadly similar, but the content is amended to foster developmentally appropriate parent-baby activities and includes "a structured group discussion using created scenarios, a shared meal, [and] a grandmothers' mini therapy group" ([Fletcher 2013](#), p 155). In Baby FAST, at least 50% of the delivery team consists of service users, with as many drawn from those who have themselves experienced teenage parenting. The team, which must be racially and culturally

representative of the community served, includes a young mother, a young father, a grandmother, a social worker, a health professional, and someone who specialises in infant massage. (ibid). Although supported by school and preschool systems, Baby FAST is geared towards supporting parents to help prepare their children for learning. It is often used as an adjunct to home visiting programmes and

- unlike other versions of FAST - is normally provided in community settings other than schools. When directed specifically at teenage mothers, it is referred to as Teen FAST.

The aims of each version of FAST for both the child and the family are shown below in their respective tables.

#### Child - Explicit FAST core outcomes for the child across FAST versions

FAST aims	FAST version				
	Baby	Pre-K	Kids	Middle	Teen
Improve interaction with education and scholastic outputs	X	X	X	X	-
Reduce unhealthy behaviours	X	X	X	X	X
Reduce stress, aggression, and violence	-	-	X	X	X
Improve self-esteem and coping skills	-	-	X	X	X

#### Family - Explicit FAST core outcomes in the family realm across FAST versions

FAST aims	FAST version				
	Baby	Pre-K	Kids	Middle	Teen
Reduce compulsive/impulsive behaviours (aggression, anxiety, depression)	X	X	X	X	X
Reduce conflict and stress	X	X	-	-	-
Reduce substance abuse	-	-	X	X	X
Improve parenting skills	X	X	-	-	-
Improve family cohesion	X	-	X	X	X
Improve communications	X	X		X	X
Increase child development and learning environment	X	X	X	X	X
Improve parental self-esteem and coping skills	X	X	-	-	-
Improve community/social capital	-	X	X	X	X

#### Recruitment

In each school where FAST is offered, when a teacher identifies a child at risk, he or she informs the parent of these concerns and the availability of the FAST programme. This 'targeted recruitment' is followed by a 30-minute home visit from a member of the FAST staff,

accompanied by a parent graduate. [Layzer 2001](#) argued that, due to the highly structured nature of the FAST home visit, the recruitment process itself could be considered to be part of the intervention. In Baby FAST, parents are enrolled by means of outreach programmes in the community, as well as by open enrolment.

## Training and delivery

The programme requires that the implementation site must have one certified trainer for each FAST team. Training consists of four days' training over four months, including two days' attendance at workshops, three site visits, and one review day. Family size varies across studies from between two and 'nine or more' children (McDonald 2009a): for example, in McDonald 2010, the average number of children per family was 4.9, and in Crozier 2010, it was 2.4.

With the exception of Baby FAST, the programme is delivered on school premises. Baby FAST sessions take place in community locations such as churches and clinical and children's centres.

## How the intervention might work

FAST was developed to address the limitations of unidimensional (school or home) and unidirectional (school to home) inter-

ventions. With children spending, on average, 30 hours a week at school and 138 hours within their family and community, an intervention that recognised the relatively limited influence that schools can maintain and aimed to capture the influence of wider family and social networks was considered to have greater potential to sustain positive outcomes (see above). Coote 2000 describes the main working characteristics of FAST as an early intervention, ideally reaching at-risk children early on in their life course, but also providing strategic support at key transition points such as adolescence. As detailed in the programme's logic model (Figure 1), FAST is designed to promote prosocial development in children and families by creating a collaborative support system of family, school, peers, community, and professionals, to develop protective behaviours that promote family resilience and prevent maladaptive behaviours from becoming entrenched.

**Figure 1. Fast logic model.** Copyright © 2016 Families and Schools Together, Inc.: reproduced with permission.



FAST aims to nurture high levels of participation and completion of the intervention by encouraging voluntary participation and supportive networks, combined with incentives and structured, enjoyable, interactive group programmes. Family support may include transport to FAST meetings, a meal, child care, prize winning, and access to social supports. Engagement is encouraged through interactive tasks with clear learning goals, and learning takes place in an atmosphere of mutual support rather than passive parental education or training. Conflict and criticism are actively discouraged, whilst positive and supportive behaviours, including the establishment of parental roles, are reinforced through repetition and task completion. Family members are encouraged to act out and

discuss their emotions through a positive 'Feelings Charades' activity, designed to break down barriers and facilitate talking openly in family groups about each individual's feelings. A short time is set aside during each session for one-to-one play sessions ('Special Play Time'), where adults must let the child lead the play activity and refrain from bossing, teaching, or directing the child, instead learning to let the child direct their time together.

Parents are encouraged to strengthen their relationships with each other by communicating during 'Buddy Time' - a session that allows adults to talk about their day in a controlled, child-free environment, supported by the following 45-minute, parental self-



help group exercise. Family cohesion is fostered through family group tasks such as developing a family flag, cooking a family meal, singing, and participating in structured, family communication games. At the end of the FAST session, community is reinforced through announcements (birthdays or other notable events) and a closing ritual. Once families have completed the eight-week programme, a graduation ceremony is held within the school, and family and friends are encouraged to attend and support the graduating families. Once completed, the further two years of support provided through the FASTWORKS programme is designed to help maintain and further develop progress.

### Why it is important to do this review

Several randomised controlled trials (RCTs) have evaluated the effectiveness of FAST, but, to date, no systematic review of FAST has been conducted to summarise and critically appraise this evidence. This review aims to fill this gap.

## OBJECTIVES

To assess the effectiveness of the Families and Schools Together (FAST) programme in improving outcomes among children and their families.

## METHODS

### Criteria for considering studies for this review

#### Types of studies

Randomised controlled trials (RCTs) and quasi-RCTs (in which methods of allocation to groups are not truly random, e.g. day of week, case number).

#### Types of participants

Families with children from birth to age of completion of compulsory education from all ethnic backgrounds and family sizes, however defined by the trialists.

#### Types of interventions

FAST programmes compared with waiting list, usual services, alternate service, or no treatment.

#### Types of outcome measures

We assessed outcome measures (1) immediately after completion of the intervention (zero to two months), (2) at short-term follow-up (three to nine months), and (3) at long-term follow-up (10+ months after completion of the intervention).

We recorded the timing of outcome assessment as presented in studies.

#### Primary outcomes

##### Child outcomes

1. Improved school performance\*, as measured by grades or marks that students earn, standardised educational tests, performance tests, or other objective measure of educational attainment. Grades/marks that describe academic performance in at least two classes in the same time frame are eligible (e.g. grade in a math course is not eligible, average grade in academic courses is eligible, average grade across all classes is eligible). Grades/marks in a single course are not eligible.

ic courses is eligible, average grade across all classes is eligible). Grades/marks in a single course are not eligible.

2. Adverse outcomes. Any reported increase in targeted negative child behaviours or conversely reported decrease in promoted positive behaviours, including school performance (which may be indicative of group contagion effect).

##### Parent outcomes

1. Reduced parental substance use\*, as measured by any standardised self-report or objective measure of substance use, not including indirect attitude, perception, or awareness measures (Foxcroft 2011).
2. Reduced parental stress\*, as measured by any standardised measure of parental stress such as the Parenting Stress Index (Loyd 1985).

##### Secondary outcomes

##### Child outcomes

1. Improved internalising behaviours or symptoms at school or at home\*, as recorded on a standardised measure such as the internalising subscale of the Child Behaviour Checklist (Achenbach 1991), or on a similar standardised measure.
2. Improved externalising behaviours or symptoms at school or at home\*, as recorded on a standardised measure such as the externalising subscale of the Child Behaviour Checklist (Achenbach 1991), or on a similar standardised measure.
3. Reduced substance use\*, as measured for example by any self-report or objective measure of alcohol consumption, including quantity, frequency, or incidence of drunkenness (Foxcroft 2011).
4. Increased school attendance, as measured by any objective record of school attendance such as a school or class register.
5. Reduced youth delinquency, as measured by self-reports or official records of contacts with the juvenile justice system or another similar law enforcement agency.

##### Parent outcomes

1. Increased parental self-efficacy\*, as measured by improved scores on a standardised measure of parental self-efficacy such as the Self-Efficacy for Parenting Tasks Index (SEPTI; Coleman 2000), or on a similar standardised instrument.
2. Improved parent-child relationship\*, as measured by improved scores on a standardised measure of the parent-child relationship such as the Parent-Child Relationship Inventory (PCRI; Gerard 1994), or on a similar standardised measure.
3. Increased parental engagement with education\*, as measured by both teachers' and parents' reports of parental involvement with education, including attendance at school-based events, correspondence between parent and teacher, parental engagement with homework, learning activities, and educational materials, as well as extracurricular activities, objective measures of parental values and attitudes towards education, and the aspirations they have for their child's development.
4. Increased parental uptake of services (mental health, drug and alcohol), as measured through reported referrals to, or attendance at, mental health, drug, and/or alcohol services.
5. Increased parental involvement in community-based activities.

## Family outcomes

1. Improved family relationships\*, as measured by improved scores on a standardised measure of family relationships such as the Family Environment Scale (FES; [Moos 1994](#)), or on a similar standardised instrument.
2. Reduction in child abuse and neglect, as measured by reduced incidence of child maltreatment on a standardised measure of child abuse and neglect such as the Juvenile Victimization Questionnaire (JVQ; [Finkelhor 2005](#)), or on a similar standardised measure, or by official records from law enforcement or social welfare agencies.

We will use those outcomes marked with an asterisk to populate the 'Summary of findings' table for this review.

## Search methods for identification of studies

We conducted electronic searches of bibliographic databases, government policy databanks, and professional websites using the search strategies in [Appendix 1](#). We did not apply any geographical, language, or publication restrictions, and we planned to seek translations for reports published in languages other than English. We confined our searches to information post 1988 - the year that FAST was developed.

## Electronic searches

We searched the following electronic resources in February 2017, and we updated the searches between 19 October and 14 December 2018.

1. Cochrane Central Register of Controlled Trials (CENTRAL; 2018 Issue 12), in the Cochrane Library, which includes the Cochrane Developmental, Psychosocial, and Learning Problems Specialised Register (searched 14 December 2018).
2. MEDLINE Ovid (1946 to 19 October 2018).
3. Embase Elsevier (1980 to 24 October 2018).
4. PsycINFO EBSCO (1806 to 19 October 2018).
5. Education Resources Information Center (ERIC) EBSCOhost (1966 to 19 October 2018).
6. British Education Index (BEI) EBSCOhost (1950 to 13 November 2018).
7. ProQuest Education Database (1988 to 13 November 2018).
8. Education Abstracts (HW Wilson) EBSCOhost (1983 to 13 November 2018).
9. Social Science Citation Index Web of Science (1970 to 24 October 2018).
10. Conference Proceedings Citation Index Social Science and Humanities Web of Science (1990 to 13 November 2018).

11. Evidence for Policy and Practice Information (EPPI) Centre Database of Education Research ([eppi.ioe.ac.uk/webdatabases/Intro.aspx?ID=6](http://eppi.ioe.ac.uk/webdatabases/Intro.aspx?ID=6)) (searched 5 November 2018).
12. Campbell Library of Systematic Reviews ([www.campbellcollaboration.org/library.html](http://www.campbellcollaboration.org/library.html)) (searched 14 December 2018).
13. *Cochrane Database of Systematic Reviews* (CDSR; 2018 Issue 12), part of the Cochrane Library (searched 14 December 2018).
14. Database of Abstracts of Reviews of Effectiveness (DARE; [www.crd.york.ac.uk/CRDWeb/](http://www.crd.york.ac.uk/CRDWeb/)) (searched 5 November 2018).
15. Epistemonikos ([www.epistemonikos.org](http://www.epistemonikos.org)) (searched 13 December 2018).
16. UK Clinical Trials Gateway ([www.ukctg.nihr.ac.uk/clinical-trials](http://www.ukctg.nihr.ac.uk/clinical-trials)) (searched 13 December 2018).
17. ClinicalTrials.gov ([clinicaltrials.gov](http://clinicaltrials.gov)) (searched 13 December 2018).
18. World Health Organization International Clinical Trials Registry Platform (WHO ICTRP; [www.who.int/ictip/en](http://www.who.int/ictip/en)) (searched 14 December 2018).

## Searching other resources

We examined the reference lists of reports, reviews, and primary studies, and we contacted the FAST programme developer and independent researchers to identify studies not retrieved by electronic searches. In addition, we searched the Families and Schools Together website ([familiesandschoolstogether.com](http://familiesandschoolstogether.com)), the US Department of Education's What Works Clearinghouse ([ies.ed.gov/ncee/wwc/FWW](http://ies.ed.gov/ncee/wwc/FWW)), the US Department of Education's grant awards ([www.ed.gov/category/subject/grants](http://www.ed.gov/category/subject/grants)), and the UK's Education Endowment Foundation's projects and evaluation page ([educationendowmentfoundation.org.uk/projects-and-evaluation](http://educationendowmentfoundation.org.uk/projects-and-evaluation)).

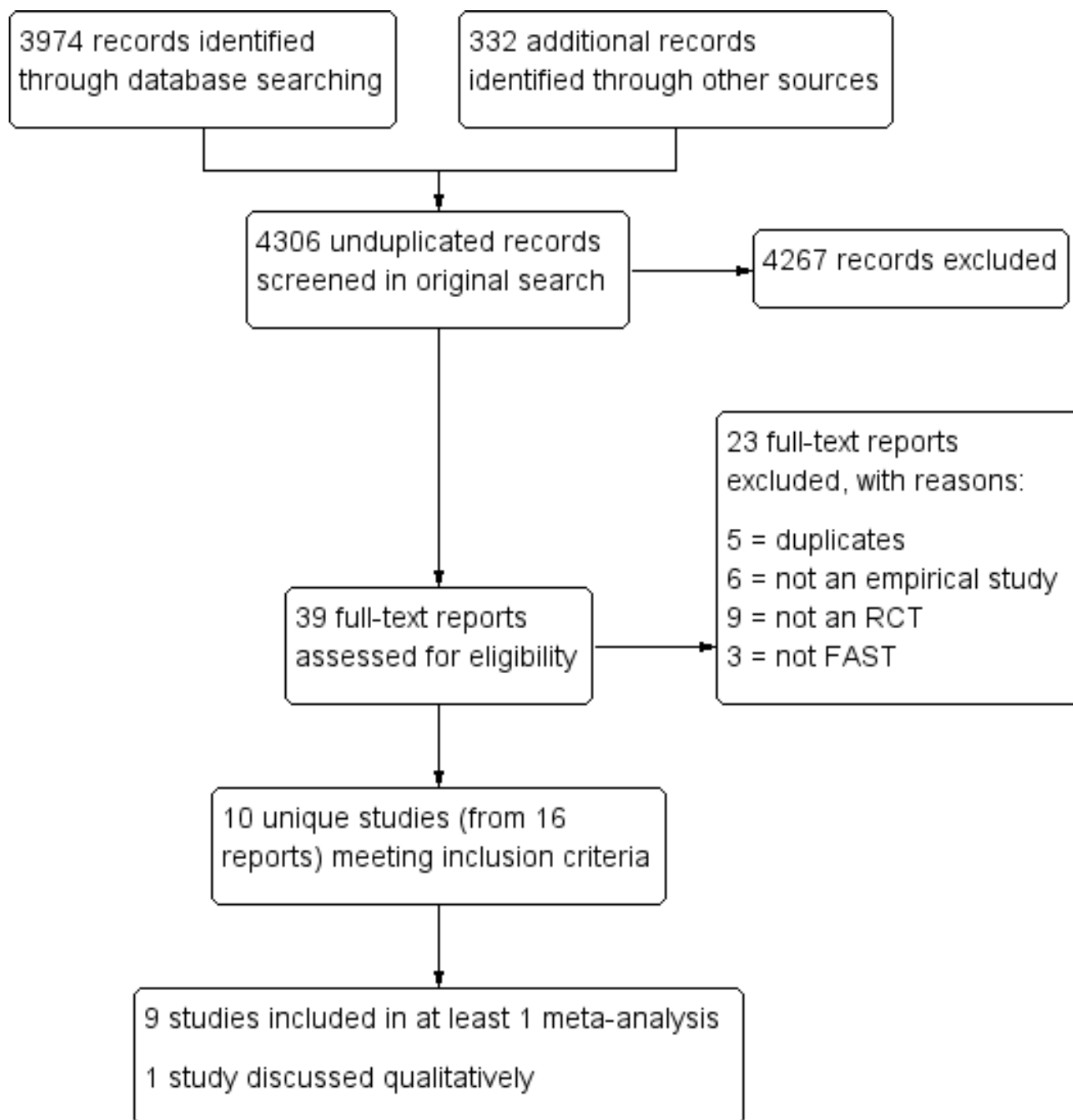
## Data collection and analysis

We used Review Manager 5 (RevMan 5) to organise and analyse our data ([Review Manager 2014](#)). We used EndNote to manage our bibliographical data ([EndNote 2013](#)).

## Selection of studies

At least two review authors independently reviewed all titles and abstracts to determine potentially relevant studies. We retrieved the full text of any citation that was identified by at least two review authors as potentially relevant (the two review authors worked independently, and a third review author (usually JV) reconciled disagreements). At least two review authors independently read all retrieved papers of potentially relevant studies to determine whether they satisfied the inclusion criteria ([Criteria for considering studies for this review](#)). Review authors resolved disagreements by discussion with JV and GM, as needed. When we excluded a retrieved study, we documented the reasons for its exclusion (see [Figure 2](#) and [Characteristics of excluded studies](#)).

**Figure 2. Study flow diagram for the literature search.**



#### Data extraction and management

For each included study, one review author (JV), along with a second independent review author (AF, DK, or SL), extracted and recorded all relevant data on a specifically designed and piloted data collection form. Review authors consulted with one another to resolve disagreements, as needed. We extracted the following data.

1. Study characteristics: study author(s), year of publication, journal or source, contact details, study design, study duration, attrition details, language.
2. Child characteristics: age, gender, ethnicity, special educational needs or disability.
3. Parent characteristics: age, gender, ethnicity, educational attainment or qualifications or both, employment status.
4. Family characteristics: family size, marital status, annual income.



5. School characteristics: population served, size, other interventions, location.
6. Outcomes and measures used: details on all primary and secondary outcome measures, including measures used, length of follow-up, summary data, means, and standard deviations.
7. Cost incurred by the intervention.

We collected information on study design and implementation in a format suited to completion of the 'Risk of bias' tables (Higgins 2011a). We collected raw (unadjusted) results in preference to adjusted results, for reasons of consistency of interpretation across studies, and because this choice of analysis appears to be less susceptible to selective reporting bias (e.g. it prevents potentially biased selection of covariates for inclusion in the model). This decision may, however, increase the risk of bias that may be attributable to baseline differences, such as those arising from differential dropout.

### Assessment of risk of bias in included studies

We assessed the risk of bias of included studies using Cochrane's 'Risk of bias' tool (Higgins 2011b). For each included study, two review author pairs independently assessed the risk of bias within each included study based on the seven domains listed below, with review authors' judgements presented as 'high', 'low', or 'unclear' risk of bias (see Table 1). When disagreements occurred between the judgements of review authors, they first sought to resolve these disagreements themselves. If needed, they consulted a third review author (usually JV, unless he was one of the review authors, in which case GM was consulted).

1. **Random sequence generation.** We describe the methods used to generate the allocation sequence in detail, to assess whether it was likely to produce comparable groups of participants. The question: was the allocation sequence adequately generated?
2. **Allocation concealment.** We describe the methods used to conceal the allocation sequence in detail, to determine whether intervention allocation has been concealed before and during the allocation process. The question: was the allocation adequately concealed?
3. **Blinding of participants and personnel.** Given the nature of the intervention, it is not possible to blind participants or personnel to knowledge of the allocated intervention, and we examine the extent to which this may have introduced high risk of bias. The question: was performance biased due to knowledge of the allocated interventions by participants and personnel during the study?
4. **Blinding of outcome assessment.** We provide a description of the methods used to blind outcome assessors to knowledge of the allocated intervention to ascertain whether adequate protection of concealment was maintained throughout the study. The question: was knowledge of the allocated intervention adequately prevented during the study?
5. **Incomplete outcome data.** We describe the completeness of outcome data for each main outcome, noting reported attrition and exclusions in each intervention group, reasons for attrition or exclusion, and any re-inclusions in analysis employed by the review authors. The question: were incomplete outcome data adequately addressed?
6. **Selective outcome reporting.** We examine the comprehensiveness of outcome reporting in relation to published reports or available study protocols to ascertain whether selective outcome reporting was employed. The question: are reports of the study free from selective outcome reporting?
7. **Other sources of bias not addressed under the preceding domains.** We examined study protocols in sufficient detail to ascertain whether other sources of bias are present. The question: are reports of the study free from the sources of bias listed in the table below?

### Other potential sources of bias

Design bias	Description of effects
Cluster-randomised trials	Recruitment bias
	Baseline imbalance
	Loss of clusters
	Incorrect analysis
	Comparability with RCT
Early stopping of trial	Results showing statistically significant, large effects
	Occurrence of harm
	Stopping of study contrary to protocol
Randomised block design	In certain circumstances, compromised selection bias and blinding caused by the blocking process

## Measures of treatment effect

Where possible, we calculated intervention effects using Cochrane's software, RevMan 5 ([Review Manager 2014](#)).

### Dichotomous data

With one exception, all outcomes were measured on a continuous scale. The exception was [Layzer 2001](#) (see [Analysis 7.3](#)), in which data were presented as a proportion of parents scoring "high" on subscales that assess family environment. We computed an odds ratio for these effects, then performed a Cox transformation to arrive at an approximate standardised mean difference effect size (and its standard error).

### Continuous data

We calculated standardised mean differences (SMDs) (because studies used different measures to assess the same outcomes) and 95% confidence intervals (CIs).

### Multiple outcomes

When an included study provided multiple, interchangeable measures of the same construct at the same point in time (e.g. multiple measures of the quality of family relationships), we calculated the average SMD across all relevant outcomes and the average of their estimated variances. This strategy is intended to avoid the need to select a single measure, and to avoid inflated precision in any meta-analyses that might arise from placing more weight on studies that report on more than one outcome measure than on others that rely on a single measure. Studies were too few to support robust variance estimation.

Some studies measured outcomes at multiple points in time. We analysed outcomes separately for the following three time periods: (1) immediately after completion of the intervention (zero to two months), (2) short-term follow-up (three to nine months), and (3) long-term follow-up (10+ months after completion of the intervention).

### Economic issue

We recorded any costs incurred by the FAST programme reported within the studies under review.

### Unit of analysis issues

#### Cluster-randomised trials

Several of the studies included in this review are cluster-randomised trials, and sometimes, study authors analysed data from cluster trials as if the *participants* were randomly assigned to conditions. In these cases, the standard error of the test statistic can be too small because it ignores systematic similarity between participants within the same cluster. One implication of this is that the probability values that arise from the hypothesis tests can be too small. We planned on implementing a correction to account for this (see [Macdonald 2017](#); [Table 2](#)); the correction was needed for several studies in this review. However, we did not implement this correction because doing so would not have changed the conclusions of the review in any way. Most of the effect sizes in the included studies already were not statistically significant, and the three that were statistically significant remained so even after the most conservative cluster correction that we proposed in the protocol (i.e. intraclass correlation coefficient (ICC) = 0.10). Implementing

the cluster correction would similarly not have affected the results of the meta-analyses that we conducted (although in some cases, the 95% CIs would have been somewhat wider).

### Studies with multiple treatment groups

We did not find any studies that used multiple treatment groups. See our protocol - [Macdonald 2017](#) - and [Table 2](#) for methods for handling of studies with multiple treatment groups in future updates of this review.

### Dealing with missing data

Where necessary, we contacted the corresponding authors of included studies to secure any unreported data (e.g. group means and standard deviations, details of dropouts, reasons for attrition). We also contacted other study authors as necessary. One study reported outcomes only for those participants who were considered to have graduated FAST ([McDonald 2012b](#)). We attempted to obtain the additional information necessary to facilitate analyses according to intention-to-treat (ITT) principles, but the data were not available. We describe missing data and attrition/dropout rates for each included study in the 'Risk of bias' tables and discuss the extent to which missing data could affect the results of the review or the conclusions drawn.

### Assessment of heterogeneity

We assessed and described clinical variation across included studies (variability in participants, the FAST programme) and methodological diversity (randomisation, randomisation concealment, blinding of outcomes assessment, losses to follow-up, etc.). We described statistical heterogeneity by computing the  $I^2$  ([Deeks 2011](#); Section 9.5) - a quantity that broadly describes the proportion of variation in point estimates that is due to heterogeneity rather than to sampling error. In addition, we used a  $\chi^2$  test of homogeneity to determine the strength of evidence that heterogeneity is genuine. Inconsistency between studies may be ambiguous and depends upon several factors; therefore results of the  $I^2$  test may be roughly interpreted as follows.

1. 0% to 40% might not be important.
2. 30% to 60% may represent moderate heterogeneity.
3. 50% to 90% may represent substantial heterogeneity.
4. 75% to 100% represents considerable heterogeneity.

### Assessment of reporting biases

We were unable to investigate the potential impact of reporting bias by using funnel plots, as the maximum number of studies contributing to any outcome was five. See our protocol - [Macdonald 2017](#) - and [Table 2](#) to learn how we will manage this if future updates permit.

### Data synthesis

We synthesised the data using RevMan 5 ([Review Manager 2014](#)). All overall effects were calculated using inverse variance methods. As planned, we used both a fixed-effect model and a random-effects model and compared the two to assess the impact of statistical heterogeneity. We present the results from the fixed-effect model, given that the review is focused on one intervention. See our protocol - [Macdonald 2017](#) - and [Table 2](#) for further details of methods planned but not implemented in this first review of FAST.

## 'Summary of findings' table

We present the main findings of the review in a 'Summary of findings' table, developed using the GRADEpro Guideline Development Tool (GRADEpro GDT 2017). [Summary of findings for the main comparison](#) briefly describes the population, setting, intervention, and comparison/control for each included study before setting out a summary and assessment of the certainty of the main results, overall completeness and applicability of evidence, certainty of the evidence, and potential sources of bias for each outcome (Schünemann 2011a; Section 11.5.6).

Using the GRADE approach (Schünemann 2011b; Section 12.2), two review authors independently graded the certainty of evidence as high, moderate, low, or very low, according to the presence of the following five factors: limitations in design and implementation of available studies; indirectness of evidence; inconsistency of results; imprecision of results; and high likelihood of publication bias.

As empirical evidence suggests that relative effect measures are both more consistent and more inclined to be understood and used by practitioners, we planned to present odds ratios (ORs) for dichotomous data in the 'Summary of findings' table in terms of a percentage risk ratio reduction; however, all outcomes were measured on a continuous scale.

## Subgroup analysis and investigation of heterogeneity

Studies were too few to support any of our planned subgroup analyses in this first systematic review of FAST. Please see our protocol - [Macdonald 2017](#) - and [Table 2](#) for our published strategy on the exploration of potential effect modifiers of FAST.

## Sensitivity analysis

We did not conduct any sensitivity analyses in this review because (1) all studies are RCTs, (2) there did not appear to be outliers, (3) all of the meta-analytical weights are within reason, and (4) studies were too few to support an analysis of cluster RCTs versus individually assigned RCTs. Please see our protocol - [Macdonald 2017](#) - and [Table 2](#) for sensitivity analyses planned but not implemented in this review.

# RESULTS

## Description of studies

### Results of the search

The searches generated 4306 records in total. We excluded 4267 records at title and abstract stage, and we obtained the full texts of 39 papers that merited closer inspection. Assessment of the full texts of each of these led to the inclusion of 16 reports (10 studies; see [Included studies](#)). We excluded 23 reports, 18 with reasons (see [Excluded studies](#)). The five remaining reports were duplicates of studies that we had already screened. See [Figure 2](#).

### Included studies

We included 10 unique RCTs in this review ([AIR 2018](#); [Billingham 1993](#); [Knox 2011](#); [Kratochwill 2004](#); [Kratochwill 2009](#); [Layzer 2001](#); [Lord 2018](#); [McDonald 2012b](#); [Moberg 2007](#); [López Turley 2017](#)).

We provide further details about the included studies below and in the [Characteristics of included studies](#) tables.

## Study design

All 10 studies were RCTs. Five of the 10 studies were cluster-RCTs ([AIR 2018](#); [Knox 2011](#); [López Turley 2017](#); [Lord 2018](#); [Moberg 2007](#)).

## Settings and locations

With the exception of one study that took place in the UK ([Lord 2018](#)), all studies were conducted in the USA. Of the USA-based studies, all except [Kratochwill 2004](#) and [McDonald 2012b](#) took place in urban environments.

In [McDonald 2012b](#), the intervention took place at a community refugee centre, and in [Knox 2011](#), the intervention took place at a community-based health centre. [Layzer 2001](#) is unclear about where the intervention took place: "at the school (or other community center)" (quote, p B2-2). All other studies were implemented in the schools attended by children identified by teachers for inclusion in FAST.

## Sample size

Analysis sample sizes varied widely across studies, ranging from two studies with samples of 10 - [McDonald 2012b](#) - and 32 - [Billingham 1993](#) - children, to two studies with 2832 - [López Turley 2017](#) and 4221 children - [Lord 2018](#). Over 9000 children and their families contributed to at least one outcome assessed in this review.

## Participants

Children were five to nine years old (mean age approximately six years).

The race/ethnicity of participants was not uniformly reported in studies, but at least 62% of children in the USA-based studies were from an ethnic minority group (largely African American or Latino). About half of children in the samples were girls. Most children were considered to be at risk (vaguely defined), and only one study explicitly recruited children who were believed to be at risk for needing special education services ([Kratochwill 2009](#)). The socio-economic status of the families was not well described, but given the populations served, it is presumably low in most studies (in [Lord 2018](#), to be eligible for the study, schools must have had more than 20% of students eligible for free school meals). When socio-economic status was described, it was described as low for most of the families in the sample.

## Interventions and comparisons

Given the ages of the children, all included studies evaluated what is sometimes referred to as 'Kids FAST' and sometimes 'Elementary Level FAST', although neither name was used in the papers, which simply described the intervention as "FAST".

All studies appear to have had some involvement from either the intervention developer (Dr McDonald is credited with authorship on four of the 10 RCTs) or the FAST organisation (e.g. by providing training to programme implementers). This led to rather more consistency in the FAST programme elements than might be typical for a complex social intervention. In general, FAST was intended to be implemented in eight weekly, multi-family group sessions lasting two and a half to three hours. All FAST implementations had a similar structure (e.g. had structured time for group family therapy and parent-child interaction), but the details of these activities differed somewhat across sites.

In general, the experiences of the comparison students and families were not well articulated across studies. Two studies compared FAST to a presumably ineffective intervention ([Layzer 2001](#) and [Moberg 2007](#) provided literature on parenting). Two studies used a wait-list control condition ([AIR 2018](#); [McDonald 2012b](#)). One study described the control condition as "business as usual" ([López](#)

[Turley 2017](#)). The rest of the studies did not describe the control condition.

### Primary outcomes

Of the two primary outcomes related to children, five studies provided data on improved school performance. How these were measured in each of the three studies is summarised in the following table.

Measures of primary outcomes from included studies		
Outcome domain	Measure	Study
Child school performance*	Peabody Picture Vocabulary Test 4	<a href="#">AIR 2018</a>
	Woodcock-Johnson III Tests of Achievement: Literacy Subscale	
	Woodcock-Johnson III Tests of Achievement: Mathematics Subscale	
	Curriculum-based measure: Reading	<a href="#">Kratochwill 2004</a>
	Curriculum-based measure: Math	
	Grades earned (average of all academic subjects)	<a href="#">Layzer 2001</a>
	Key Stage 1 Test (reading and arithmetic)	<a href="#">Lord 2018</a>
	State of Wisconsin Standardised Test: Reading Raw Score	<a href="#">Moberg 2007</a>
	State of Wisconsin Standardised Test: Reading Scale	
	State of Wisconsin Standardised Test: Mathematics Scale	
	State of Wisconsin Standardised Test: Science Scale	

\*Outcomes included in [Summary of findings for the main comparison](#).

No study reported any adverse outcomes.

No study assessed the impact of FAST on either of the two primary outcomes related to parents, namely, reductions in substance mis-

use (measured by standardised self-report or objective measures) or parental stress.

### Secondary outcomes

As the following table indicates, four studies provided data on three of the five secondary child outcomes, three of which also provided data on two of the five secondary outcomes related to parents.

Measures of secondary outcomes from included studies		
Outcome domain	Measure	Study
Child internalising symptoms*	Revised Child Behavior Checklist: Parent Form	<a href="#">Billingham 1993</a>
	Revised Child Behavior Checklist: Teacher Form	
	Child Behaviour Checklist (CBCL) - Internalising: Parent Form	<a href="#">Kratochwill 2004</a>
	Child Behaviour Checklist (CBCL) - Internalising: Teacher Form	

Child externalising symptoms*	Social Competence and Behavior Evaluation Scale ( <a href="#">LaFreniere 1996</a> ) (note, used 10 items that focused on child aggressive behaviour (see p 69), 5 items adapted from <a href="#">Musher-Eizenman 2004</a> , and 7 items focusing on self-reported aggressive behaviour from the Metropolitan Area Child Study ( <a href="#">MACS 2002</a> ))	<a href="#">Knox 2011</a>
	Child Behavior Checklist (CBCL) - Externalising: Parent Form	<a href="#">Kratochwill 2004</a>
	Child Behavior Checklist (CBCL) - Externalising: Teacher Form	
Parent engagement with education*	Family Involvement Questionnaire: Home-Based Involvement Subscale	<a href="#">AIR 2018</a>
	Family Involvement Questionnaire: School-Based Involvement Subscale	
Family relationships*	Child-Parent Relationship Scale: Closeness Subscale	<a href="#">AIR 2018</a>
	Child-Parent Relationship Scale: Conflict Subscale	
	Family Adaptability and Cohesion Evaluation Scale: Cohesion Subscale	<a href="#">Billingham 1993</a>
	Family Adaptability and Cohesion Evaluation Scale: Adaptability Subscale	<a href="#">Kratochwill 2004</a>
	Family Adaptability and Cohesion Evaluation Scale: Cohesion Subscale	

\*Outcomes included in [Summary of findings for the main comparison](#).

No study measured the effects of FAST on children's substance use or delinquency, or on increased parental self-efficacy, parental uptake of services by parents, or their increased involvement in community-based activities.

### Funding sources

All but two studies ([Kratochwill 2009](#) and [McDonald 2012b](#)) explicitly mentioned external funding. The UK trial was funded by the Education Endowment Foundation ([Lord 2018](#)). The US studies were largely funded by federal agencies, including the National Institute of Child Health and Human Development ([López Turley 2017](#)), the Centers for Disease Control and Prevention ([Knox 2011](#)), the Department of Education ([AIR 2018](#); [Kratochwill 2004](#); [López Turley 2017](#)), the Department of Health and Human Services ([Layzer 2001](#)), and the National Institute on Drug Abuse ([Moberg 2007](#)).

State sources (Wisconsin Department of Public Instruction) funded [Billingham 1993](#).

### Excluded studies

Of the 23 studies that were excluded after full text screening, five were duplicate citations of already identified studies, six were not empirical studies, nine were not RCTs, and three were studies of an intervention other than FAST. See [Characteristics of excluded studies](#) tables.

### Risk of bias in included studies

We describe and present the risk of bias for each included study (separately by outcome, when necessary) in the [Characteristics of included studies](#) tables. Below, we present a summary of our judgments, which we also summarise graphically in [Figure 3](#) and [Figure 4](#).

**Figure 3. Risk of bias summary: review authors' judgements about each risk of bias item for each included study.**

	Random sequence generation (selection bias)	Allocation concealment (selection bias)	Blinding of participants and personnel (performance bias)	Blinding of outcome assessment (objective outcomes)	Blinding of outcome assessment (detection bias) (parent-reported outcomes)	Blinding of outcome assessment (detection bias) (teacher-reported outcomes)	Incomplete outcome data (attrition bias): low attrition outcomes	Incomplete outcome data (attrition bias): high attrition outcomes	Selective reporting (reporting bias)	Other bias
AIR 2018	+	-	-	+	-		+		+	-
Billingham 1993	?	?	-		-	?		-	?	-
Knox 2011	+	?	-		-			-	?	-
Kratochwill 2004	?	?	-	+	-	?		-	+	-
Kratochwill 2009	?	?	-	+	-	?		-	+	-
Layzer 2001	?	?	-		-	?	+	-	?	-
López Turley 2017	?	-	-			-	+		-	-

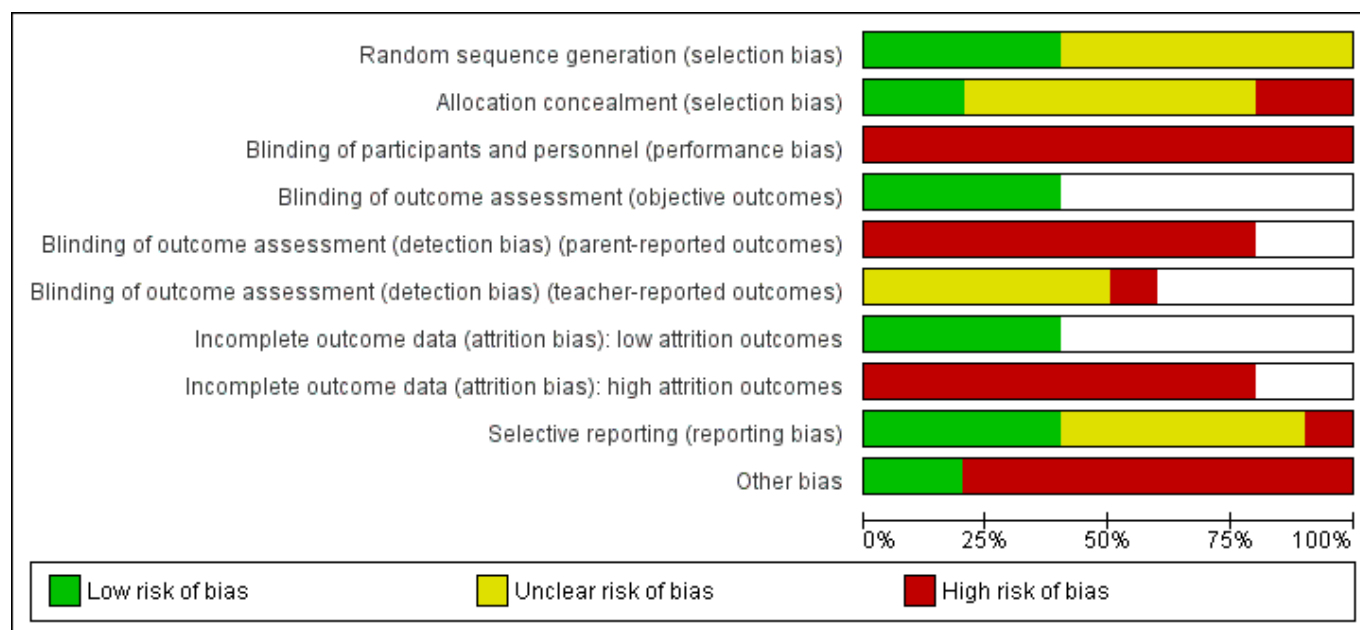
**Families and Schools Together (FAST) for improving outcomes for children and their families (Review)**

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McDonald 2012b	?	?	-		-			-	?	+
Moberg 2007	+	+	-		-	?	+	-	?	-



**Figure 4. Risk of bias graph: review authors' judgements about each risk of bias item presented as percentages across all included studies.**



## Allocation

### Random sequence generation

In terms of random sequence generation, we rated four studies at low risk of bias due to their use of adequate methods of sequence generation (AIR 2018; Knox 2011; Lord 2018; Moberg 2007). We judged the remaining six studies at unclear risk of bias because the method of sequence generation was not described.

### Allocation concealment

We judged allocation to be adequately concealed in two studies - Lord 2018; Moberg 2007 - and inadequately concealed in two studies - AIR 2018; López Turley 2017. In AIR 2018, research staff clearly had access to group assignment information during enrolment. In López Turley 2017, it is unclear whether school assignment conditions could have been foreseen in advance of or during enrolment. However, it is clear that participants and programme staff knew the assignment condition before obtaining consent, leaving open the possibility that this knowledge may have influenced the behaviour of the programme staff and/or the participants. We judged the six remaining studies to be at unclear risk of bias in this domain.

## Blinding

### Performance bias

We judged all studies in this review to be at high risk for performance bias (AIR 2018; Billingham 1993; Knox 2011; Kratochwill 2004; Kratochwill 2009; Layzer 2001; López Turley 2017; Lord 2018; McDonald 2012b; Moberg 2007), as staff were aware of condition assignments, and families in the FAST condition knew that they were participating in an intervention.

## Detection bias

### Objective outcomes

We judged objective outcomes from four studies to be at low risk of detection bias (AIR 2018; Kratochwill 2004; Kratochwill 2009; Lord 2018). We judged AIR 2018 to be at low risk for detection bias for child school attendance and child academic achievement. Attendance data were taken from school records, and child academic achievement was measured by standardised tests that appear to have been collected from all students. Similarly, child academic outcomes from Kratochwill 2004, Kratochwill 2009, and Lord 2018 were based on standardised tests that appear to have been administered to all students.

### Parent-reported outcomes

We judged all parent-reported outcomes from eight studies to be at high risk of detection bias because parents knew that they were participating in an intervention (Billingham 1993; Knox 2011; Kratochwill 2004; Kratochwill 2009; Layzer 2001; López Turley 2017; McDonald 2012b; Moberg 2007).

### Teacher-reported outcomes

We judged teacher-reported outcomes from five studies to be at unclear risk of detection bias because it is not clear whether teachers were aware of the assignment conditions of the students they were rating (Billingham 1993; Kratochwill 2004; Kratochwill 2009; Layzer 2001; Moberg 2007). We judged one study - López Turley 2017 - to be at high risk of bias because the study authors collected outcome information on students of teachers who, due to school-level randomisation, appear to have been aware of assignment conditions.

## Incomplete outcome data

We judged two studies to be at low risk of attrition bias (AIR 2018; López Turley 2017). Two studies had some outcomes for which we judged attrition bias to be low and others for which we judged attrition bias to be high (Layzer 2001; Moberg 2007). We rated the remaining six studies as having high risk of attrition bias (Billingham 1993; Knox 2011; Kratochwill 2004; Kratochwill 2009; Lord 2018; McDonald 2012b).

We judged two studies to be at low risk of attrition bias (AIR 2018; López Turley 2017). Two studies had some outcomes for which we judged attrition bias to be low and others for which we judged attrition bias to be high (Layzer 2001; Moberg 2007). We rated the six remaining studies as having high risk of attrition bias (Billingham 1993; Knox 2011; Kratochwill 2004; Kratochwill 2009; Lord 2018; McDonald 2012b).

## Selective reporting

We rated four studies as having low risk of selecting reporting bias (AIR 2018; Kratochwill 2004; Kratochwill 2009; Lord 2018). AIR 2018 appeared to have an analysis plan that was determined before data collection (p 15). We received statistical output for Kratochwill 2004 and Kratochwill 2009 on all measured outcomes. Lord 2018 was preregistered. We rated López Turley 2017 as having high risk of reporting bias as investigators did not report one measured outcome due to high levels of missing data. For all other studies, we were unable to make a judgement about the risk of reporting bias and thus rated risk as unclear (Billingham 1993; Knox 2011; Layzer 2001; McDonald 2012b; Moberg 2007).

## Other potential sources of bias

Participant recruitment presents potential risk of bias in most studies included in this review. In part, this is due to concerns about spillover effects within sites, and in part to the model's explicit theory of change. In most studies, sites were assigned to conditions, and then, within sites, families were recruited into the study. This means that programme staff and families knew their assignment conditions before giving consent to participate - knowledge that in our judgement likely affected participation decisions. We rated Lord 2018, which addressed this potential problem by planning to collect data on all children in the school, and McDonald 2012b, which assigned families to conditions after obtaining their participation consent, to be at low risk of recruitment bias. For all other studies in which recruitment bias was a potential concern, outcomes were collected only for students whose families agreed to participate, and we rated these studies as having high risk of other bias.

## Effects of interventions

See: **Summary of findings for the main comparison Families and Schools Together (FAST) compared to no intervention (or care as usual) for children and their families**

We conducted both fixed-effect and random-effects meta-analyses when there were at least two studies measuring the same outcome, using the same reporting source (e.g. parent), at the same follow-up period. We conducted a total of 14 separate meta-analyses. There were no meta-analyses in which the between-studies variance estimate ( $\tau^2$ ) was statistically significant. This is not a surprising finding given that the statistical power of this analysis is low when the number of studies is small. More revealing is that in 12 of the 14

meta-analyses, the estimated between-studies variance was zero. In these cases, the fixed-effect and random-effects models yielded identical results. The two cases in which the between-studies variance was estimated as larger than zero yielded highly similar results (and the same statistical conclusions). Below, we report the results of the fixed-effect meta-analyses.

## Primary outcomes

### Child school performance

Six studies assessed child school performance - two immediately after the intervention (200 children; Kratochwill 2004; Kratochwill 2009), and four at long-term follow-up (approximately 6276 children; AIR 2018; Layzer 2001; Lord 2018; Moberg 2007). In both meta-analyses, the effect size was small, negative, and not statistically significant.

1. Immediate post-test: SMD -0.06; 95% CI -0.34 to 0.22; low-certainty evidence; Analysis 1.1.
2. Long-term follow-up: SMD -0.02; 95% CI -0.11 to 0.08; moderate-certainty evidence; Analysis 1.2.

An additional study (120 children) assessed child school performance at follow-up (Kratochwill 2004). We could not compute an effect size because standard deviations were not reported in the paper. For both measures (reading and math), the effect was negative and was not statistically significant.

No study measured any of the other primary outcomes: child adverse events; parent stress; or parent substance abuse. See above section on 'Outcomes' under [Included studies](#).

## Secondary outcomes

### Child internalising behaviour

Six studies assessed child internalising behaviour immediately after the intervention, five using parent reports (approximately 646 children; Billingham 1993; Kratochwill 2004; Kratochwill 2009; McDonald 2012b; Moberg 2007), and five using teacher reports (approximately 3557 children; Billingham 1993; Kratochwill 2004; Kratochwill 2009; López Turley 2017; Moberg 2007). Four studies assessed child internalising behaviour at long-term follow up (Kratochwill 2004; Kratochwill 2009; Layzer 2001; Moberg 2007), using both parent reports (approximately 908 children) and teacher reports (approximately 912 children). In all cases, the effect sizes were negative (which in this case implies better internalising symptoms for FAST children and therefore a positive intervention effect), small, and not statistically significant.

1. Immediate post-test, parent report: SMD -0.05, 95% CI -0.21 to 0.10; Analysis 2.1.
2. Immediate post-test, teacher report: SMD -0.09, 95% CI -0.18 to 0.00; Analysis 2.2.
3. Long-term follow-up, parent report: SMD 0.03, 95% CI -0.11 to 0.17; Analysis 2.3.
4. Long-term follow-up, teacher report: SMD -0.06, 95% CI -0.19 to 0.07; Analysis 2.4.

We considered the certainty of the evidence to be low. See [Summary of findings for the main comparison](#).



### Child externalising behaviour

Six studies assessed child externalising behaviour immediately after the intervention, four using parent reports (approximately 627 children; [Kratochwill 2004](#); [Kratochwill 2009](#); [McDonald 2012b](#); [Moberg 2007](#)), and four using teacher reports (approximately 3557 children; [Kratochwill 2004](#); [Kratochwill 2009](#); [López Turley 2017](#); [Moberg 2007](#)). Four studies assessed child externalising behaviour at long-term follow-up ([Kratochwill 2004](#); [Kratochwill 2009](#); [Layzer 2001](#); [Moberg 2007](#)), using both parent reports (approximately 754 children) and teacher reports (approximately 912 children). In all cases, the effect sizes were negative (which in this case implies better externalising symptoms for FAST children and therefore a positive intervention effect) and small.

1. Immediate post-test, parent report: SMD -0.04, 95% CI -0.20 to 0.12; [Analysis 3.1](#).
2. Immediate post-test, teacher report: SMD -0.02, 95% CI -0.11 to 0.07; [Analysis 3.2](#).
3. Long-term follow-up, parent report: SMD -0.19, 95% CI -0.32 to -0.05; [Analysis 3.3](#).
4. Long-term follow-up, teacher report: SMD -0.10, 95% CI -0.24 to 0.04; [Analysis 3.4](#).

We considered the certainty of the evidence to be low. See [Summary of findings for the main comparison](#).

One study assessed child externalising behaviour using three different measures: one parent report and two child reports (282 children; [Knox 2011](#)). We were unable to include these data in our meta-analysis because we lacked sufficient information to compute effect sizes for these outcomes. The study authors reported no statistically significant differences between the FAST and control groups for any of these measures.

### Child school attendance

Three studies assessed child school attendance: one at short-term follow-up (1361 children; [AIR 2018](#)) and two at long-term follow-up (1430 children; [AIR 2018](#); [Layzer 2001](#)). Both effect sizes were small and were not statistically significant.

1. Short-term follow-up: SMD 0.02, 95% CI -0.11 to 0.15; [Analysis 4.1](#).
2. Long-term follow-up: SMD 0.03, 95% CI -0.10 to 0.16; [Analysis 4.2](#).

We considered certainty of the evidence to be moderate.

### Parent engagement with education

Three studies assessed parent engagement with education: one immediately after completion of the intervention (approximately 473 children; [Moberg 2007](#)), one at short-term follow-up (1050 children; [AIR 2018](#)), and two at long-term follow-up (1232 children; [AIR 2018](#); [Layzer 2001](#)). The effect size observed in the one study at immediate post-test was possibly large enough to be meaningful, and the CI included values that, if real, were likely to be meaningful (SMD 0.34, 95% CI 0.15 to 0.53; [Analysis 5.1](#)). The effect size observed in one study at short-term follow-up was probably not large enough to be meaningful, but the CI included values that, if real, were possibly large enough to be meaningful (SMD 0.20, 95% CI 0.06 to 0.33; [Analysis 5.2](#)). The effect size observed in two studies that assessed parent engagement with education at long-term follow-up

was very small, and its CI did not include values that are meaningful (SMD 0.03, 95% CI -0.07 to 0.12; [Analysis 5.3](#)).

### Parent involvement in community-based activities

One study assessed parent involvement in the community at long-term follow-up (386 children; [Layzer 2001](#)). The effect size was small (SMD 0.11, 95% CI -0.15 to 0.38; [Analysis 6.1](#)), and the CI contained values that, if real, were possibly meaningful.

### Family relationships

Seven studies assessed the quality of the family relationship: four immediately after completion of the intervention (183 children; low-certainty evidence; [Billingham 1993](#); [Kratochwill 2004](#); [Kratochwill 2009](#); [McDonald 2012b](#)), one at short-term follow-up (1053 children; low-certainty evidence; [AIR 2018](#)), and four at long-term follow-up (approximately 2569 children; low-certainty evidence; [AIR 2018](#); [Kratochwill 2009](#); [Layzer 2001](#); [Moberg 2007](#)). In all cases, effect sizes were small.

1. Immediate post-test: SMD 0.03, 95% CI -0.27 to 0.33; [Analysis 7.1](#).
2. Short-term follow-up: SMD 0.00, 95% CI -0.12 to 0.13; [Analysis 7.2](#).
3. Long-term follow-up: SMD 0.08, 95% CI -0.03 to 0.19; [Analysis 7.3](#) (moderate-certainty evidence).

Of these three results, only the CI for the immediate post-test contained values that, if real, were possibly large enough to be meaningful.

No included study assessed the following secondary outcomes: child substance abuse; child delinquency; parental self-efficacy; parental uptake of services (mental health, drug, and alcohol); and child abuse and neglect.

### Costs

Two studies provided information on costs ([Kratochwill 2009](#); [Lord 2018](#)). [Kratochwill 2009](#) estimated that training FAST providers, implementing the intervention, and evaluating the impact of costs of USD 1200 per child (probably in 2002 or 2003 dollars). [Lord 2018](#) estimated the cost of FAST to schools to be £133 per child (in 2018 pounds). These estimates are quite different from one another, but [Lord 2018](#) may have benefited from economies of scale, and [Kratochwill 2009](#) included evaluation costs in their estimate.

## DISCUSSION

### Summary of main results

We identified 10 eligible randomised controlled trials (RCTs) examining the relative effects of assignment to Families and Schools Together (FAST) on child and parent outcomes. Nine of these studies (all except [Knox 2011](#), which we discuss separately) contributed to at least one meta-analysis. Of the primary outcomes, four studies assessed child school performance at long-term follow-up, but this analysis did not suggest that FAST might have an important impact. None of the studies assessed child adverse events, parent substance abuse, and parental stress. Of the secondary outcomes, only one (parent report of child externalising behaviour at long-term follow-up; [Analysis 3.3](#)) suggested that FAST might possibly have effects that are large enough to be meaningful, and had we been able to include the data from [Knox 2011](#), which also measured this outcome, the effect size would likely have been small-

er and the resulting confidence interval (CI) might not have suggested values that are possibly meaningful. Results for the outcomes of 'child internalising behaviour' and 'family relationships' suggest that FAST probably does not have an important impact on these outcomes. These conclusions are tempered by two considerations. Whilst both fixed-effect and random-effects meta-analyses produced the same results in this review, these meta-analyses were based on a small number of studies (two to five), and some of the underlying samples sizes are small. As a result, additional studies might change the point estimates. Whilst this would decrease uncertainty in fixed-effect models, it might either reduce or increase uncertainty about the estimates from random-effects models.

### Overall completeness and applicability of evidence

All but one of the included studies was conducted in urban environments in the USA and focused on children aged 5 to 11 years who could be considered to be vaguely 'at risk' (most often due to living in poverty). Given the widespread adoption of FAST, which has been implemented in more than 20 countries, the available evidence is limited. We found no trials of Baby FAST or of the versions of FAST targeted at middle or high school level students.

The studies included in this review were relatively well reported. We were able to compute effect sizes for eligible outcomes for all but one study, and the studies generally provided detailed information about FAST as it was implemented and the families receiving it.

Most trials were not large, but three studies were. [AIR 2018](#) implemented FAST in 30 schools in the USA, [Lord 2018](#) implemented FAST in 60 schools in England, and [López Turley 2017](#) implemented FAST in 26 schools in the USA. Overall, over 9000 students were included in at least one analysis in this review. Our conclusions are likely most applicable to low-income children in the USA, and perhaps in the UK. This review might be less applicable to educational contexts that differ from these, and to minority groups not represented in the review.

### Quality of the evidence

Using the GRADE approach, we rated the certainty of evidence as moderate for two outcomes, and as low for two outcomes. Reasons for these judgements are set out in the [Summary of findings for the main comparison](#). Many of the studies included in this review were at serious risk of bias. The most common problems were recruitment bias, attrition bias, and performance bias. We further downgraded two outcomes due to imprecision because the 95% CI included both potentially meaningful and beneficial, and potentially meaningful and harmful, effects of FAST.

### Potential biases in the review process

We followed standard Cochrane procedures when developing the protocol for this review ([Macdonald 2017](#)), and, whenever possible, in conducting the review. This means that we employed state-of-the-art literature search procedures in an effort to find all relevant studies examining the effectiveness of FAST.

We used multiple, trained review authors to identify potentially eligible studies and to extract descriptive information from study reports. Some details of this review were not anticipated by the protocol (see [Differences between protocol and review](#)), but we attempted to arrive at solutions without considering the analytical conse-

quences of our choices. None of the review authors have any known conflicts of interest.

### Agreements and disagreements with other studies or reviews

To our knowledge, this is the first systematic review of FAST. That said, the conclusions of our review are generally consistent with the themes that emerge from the individual studies. No studies included in this review were uniformly positive about the effects of FAST on child and parent outcomes. Instead, studies provided a mix of results, some of which were statistically significant or had CIs that contained values suggesting a potentially meaningful effect, but most of which were not statistically significant and did not have CIs suggesting a potentially meaningful effect.

## AUTHORS' CONCLUSIONS

### Implications for practice

There is currently no persuasive evidence that implementing FAST will result in important positive outcomes for assigned children and their families. Nor is there any evidence that implementing FAST will result in their experiencing any harm, although it is also the case that no study set out to monitor unintended consequences.

It might be worth considering whether it is reasonable to expect that an intervention that most commonly lasts eight weeks, with no more than 24 contact hours that are not focused on child school performance, can have a detectable impact on broad measures of child school performance. A similar point could be made about long-term measures of child behaviour and family relationships. Our sense is that it is probably not reasonable to expect that FAST might affect these outcomes, and that individuals thinking about adopting FAST should focus on outcomes more directly tied to the intervention (e.g. short-term measures of family relationships and child connection to school). It is at least possible that FAST can affect these types of outcomes, so if these are valued outcomes, FAST might be a reasonable option for intervention.

### Implications for research

It would be helpful if future researchers could consider any potential negative, unintended consequences of FAST, and measure them. In addition, we are concerned that because FAST often takes place on school grounds, parents might conflate participation in FAST with participation in schooling. Future researchers can help clarify this by using measures that explicitly distinguish between FAST participation and participation in other school-related activities.

Researchers should also reconsider the long-standing concern of FAST with spillover effects (i.e. that the benefits of FAST participation will be accrued by students in the same school who do not attend FAST). The worry about spillover effects is part of the motivation that researchers cite when justifying their choice to use a design in which clusters (e.g. schools) are randomly assigned to conditions and then, after cluster assignments are known, children and families are recruited into the intervention. While avoiding potential spillover effects, this design probably introduces recruitment bias because both recruiters and families know the conditions into which families are being recruited. Given the results of this review, spillover effects seem unlikely, and a design in which children and families are randomly assigned to conditions within schools

will likely have greater statistical power than the between-school design often utilised.

When this review is updated, it might be helpful to revise the list of eligible outcomes to focus on those that are relevant to younger children. When developing the protocol (Macdonald 2017), we anticipated finding studies that assessed the effects of FAST at multiple age ranges, but in fact, all eligible studies uncovered to date assess only effects on young children (aged five to nine years).

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\* Indicates the major publication for the study



## CHARACTERISTICS OF STUDIES

### Characteristics of included studies [ordered by study ID]

#### AIR 2018

Methods	<b>Design:</b> cluster-randomised trial, with assignment stratified on school performance level
Participants	<p><b>Location/setting:</b> Philadelphia, PA (USA)</p> <p><b>Sample size:</b> recruitment letters were sent to 7595 eligible families. Of these, 3787 (49.9%) were re-turned. Of the returned letters, 2926 families (77% of the returned letters, and 38.5% of the eligible sample) agreed to participate in the study. A random subsample of 1396 contenting families was chosen to participate in the study.</p> <p><b>Mean age:</b> approximately 5 years. We obtained data from the study authors, but these covered only a portion of the randomised sample and were not broken out by group</p> <p><b>Sex:</b> approximately 50% female</p> <p><b>Race/ethnicity:</b> African American = 52%, Latino = 21%, White = 11%, other (includes Asian American, American Indian/Alaskan Native, and students identifying as multi-racial/other) = 16%</p>
Interventions	<p><b>Intervention (analyses based on approximately 500* children):</b> FAST consisted of 8 weekly, multi-family group sessions meeting in school buildings during out of school time. Activities include "one hour of parent-led family activities...[that] may include a family craft, a family meal, family singing, or communication games...school and community staff members then organize children's time for an additional hour...next, parents practice being responsive to their children in 'special play', which is 15 minutes of one-to-one parent-child time" (quote, p 3-4)</p> <p><b>Control (analyses based on approximately 540* children):</b> no treatment, with FAST available to control participants after conclusion of the study (see p 2)</p> <p>*Sample sizes are approximate because they change depending on the outcome</p>
Outcomes	<p><b>Eligible measures (outcome domain)</b></p> <ol style="list-style-type: none"> <li>1. Peabody Picture Vocabulary Test 4 (child school performance)</li> <li>2. Woodcock-Johnson III Tests of Achievement: literacy subscale (child school performance)</li> <li>3. Woodcock-Johnson III Tests of Achievement: mathematics subscale (child school performance)</li> <li>4. School attendance (child school attendance)</li> <li>5. Family Involvement Questionnaire: home-based Involvement subscale (parent engagement with education)</li> <li>6. Family Involvement Questionnaire: school-based Involvement subscale (parent engagement with education)</li> <li>7. Child-Parent Relationship Scale: closeness subscale (family relationships)</li> <li>8. Child-Parent Relationship Scale: conflict subscale (family relationships)</li> </ol> <p><b>Ineligible measures (reason)</b></p> <ol style="list-style-type: none"> <li>1. Reciprocal support from other parents (measures perceived parent social support; not in a domain listed in the protocol for this review)</li> <li>2. Parent-Teacher Relationship Scale (measures parents' view of the quality of the parent-teacher relationship; not in a domain listed in the protocol for this review)</li> <li>3. Social Skills Improvement System Rating Scale (a single score based on teacher report using the following subscales: cooperation; assertion; responsibility; engagement; and self-control subscales; most are not domains listed in the protocol for this review)</li> </ol> <p><b>Timing of outcome assessment:</b> the first cohort of students was assessed at the end of kindergarten, at the end of first grade, and at the end of second grade. The second cohort of students was assessed at the end of kindergarten and at the end of first grade. Not all outcomes were assessed at all time points</p>

**AIR 2018** (Continued)

## Notes

**Study start date:** FAST was offered during the 2013-14 and 2014-15 school years

**Study end date:** the last data collection took place in mid-2016

**Funding source:** this trial was funded by the US Department of Education

**Conflict of interest:** none noted. The FAST organisation was one of the grantees but appears to have served as a technical assistance provider only (see p 9)

**Risk of bias**

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	<p><b>Comment:</b> this is a cluster-RCT. 60 willing and eligible schools were identified and were divided into 3 strata. The intervention was implemented in 2 successive cohorts</p> <p><b>Quote:</b> "a random number was generated for each school. Within each of the three blocks...schools were rank-ordered from the lowest to the highest, based on the random numbers assigned to them. Schools in the first half of each block were assigned to the treatment condition and the schools in the second half were assigned to the control condition" (p 14)</p>
Allocation concealment (selection bias)	High risk	<p><b>Comment:</b> research staff clearly had access to group assignment information during enrolment</p> <p><b>Quote:</b> "a random number was generated for each school. Within each of the three blocks...schools were rank-ordered from the lowest to the highest, based on the random numbers assigned to them. Schools in the first half of each block were assigned to the treatment condition and the schools in the second half were assigned to the control condition" (p 14)</p>
Blinding of participants and personnel (performance bias) All outcomes	High risk	<p><b>Comment:</b> intervention and research staff clearly knew assignment conditions. Participants in the FAST condition clearly knew that they were receiving an intervention. Parents assigned to FAST but who chose not to participate may have been less aware of this, and presumably, parents in the control schools were much less aware of the fact that they were in the control condition</p>
Blinding of outcome assessment (objective outcomes) All outcomes	Low risk	<p><b>Comment:</b> student attendance was taken from school records. Child school performance was taken from standardised tests that appear to have been administered in the usual way</p>
Blinding of outcome assessment (detection bias) (parent-reported outcomes) All outcomes	High risk	<p><b>Comment:</b> parent involvement in education; family relationship measures were completed by parents. Participants in the FAST condition clearly knew that they were receiving an intervention. Parents assigned to FAST but who chose not to participate may have been less aware of this, and presumably, parents in the control schools were likely much less aware of the fact that they were in the control condition</p>
Incomplete outcome data (attrition bias): low attrition outcomes	Low risk	<p><b>Comment:</b> across the 2 cohorts and 3 measurement periods, overall attrition ranged from 15% to 31%, with similar loss rates across FAST and control groups (the mean percentage point difference in loss was less than 4). Analyses are not contingent on programme participation</p>
Selective reporting (reporting bias)	Low risk	<p><b>Comment:</b> 1 subscale was planned to be given to parents at all 3 data collection points, but due to an administrative error, it was given only during the final data collection. The text of the report implies the presence of an analysis plan (this would be expected, given the organisation that funded this research)</p>

## AIR 2018 (Continued)

**Quote:** the analyses "were agreed upon before outcome data were collected" (p 15)

Other bias	High risk	<p><b>Recruitment bias</b></p> <p><b>Comment:</b> families were recruited after schools were randomly assigned to conditions. About 50% of the families approached to participate returned their consent forms. Of these, about 80% consented to participate (hence, about 40% of eligible families agreed to participate). FAST and control families were randomly selected from the consenting subsample</p> <p><b>Quote:</b> "for Cohort 1, we sent out recruitment materials to 2,488 families, and 1,048 (42%) returned completed consent forms. Of those returned, 796 (76%) consented to participate, and 252 (24%) declined. For Cohort 2, we sent out recruitment materials to families of all 5,107 kindergarten students in study schools, and 2,739 (54%) returned completed consent forms. Of those returned, 2,130 (78%) consented for their families to be in the study, and 609 (22%) declined. Note that we drew a random subsample of the consenting students for actual participation in the study" (p 10)</p>
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## Billingham 1993

Methods	<b>Design:</b> randomised controlled trial
Participants	<p><b>Location/setting:</b> Madison, WI (USA)</p> <p><b>Sample size:</b> "30 to 40 children" in one school "were identified as 'at-risk', and then randomly assigned" (quote, p 89). Analyses were based on 32 students and their families</p> <p><b>Mean age:</b> 7.1 years (intervention = 7.2, control = 7.0)</p> <p><b>Sex:</b> not available</p> <p><b>Race/ethnicity:</b> African American = 20%, White = 80%</p>
Interventions	<p><b>Intervention (10 students):</b> FAST involved "eight, weekly, three-hour evening sessions of multiple family group activities, facilitated by the parents themselves and a FAST team of school, mental health, and AODA specialists, and parent-advocates recruited from FAST graduates" (quote, p 13). Each session followed the same plan: "meal hosted by a family; Family Sing-Along; Structured Family Communications Exercise; Family Feelings Identification Exercise; Parent Support Meeting; Parent and Child Reflective Play Technique; Special Play; Family Doorprize; Group Closing Ritual" (quote, p 14-15)</p> <p><b>Control (22 students):</b> no information provided</p>
Outcomes	<p><b>Eligible measures (outcome domain)</b></p> <ol style="list-style-type: none"> <li>1. Revised Child Behavior Checklist - Parent (child internalising behaviours)</li> <li>2. Revised Child Behavior Checklist - Teacher (child internalising behaviours)</li> <li>3. Family Adaptability and Cohesion Evaluation Scale: cohesion subscale (family relationships)</li> </ol> <p><b>Ineligible measures (reason)</b></p> <ol style="list-style-type: none"> <li>1. Family Adaptability and Cohesion Evaluation Scale: adaptability subscale (measures "the ability of a marital/family system to change its power structure, role relationships, and relationship rules in response to situational and developmental stress" (quote, p 12); not in a domain specified in the protocol for this review)</li> <li>2. Parenting Stress Index: social isolation subscale ("The Social Isolation subscale of the PSI consists of seven questions which tap parents' (mothers') perceptions of social support" (quote, p 95); not in a domain specified in the protocol for this review)</li> </ol>

## Billingham 1993 (Continued)

3. Family support ("mothers were also asked to rate their perceived satisfaction with social support for parenting on a 5-point positive Likert scale item, 'Generally, I am satisfied with the support I receive for parenting'" (quote, p 95); not in a domain specified in the protocol for this review)
4. Pictorial Scale of Perceived Competence and Social Acceptance for Young Children ("tap[s] four subscale domains of children's perceptions: cognitive competence, physical competence, maternal acceptance, peer acceptance" (quote, p 96); none of these subscales fall into a domain specified in the protocol for this review)

**Timing of outcome measurement:** immediately after FAST participation

### Notes

**Study start date:** unclear. A different analysis in this paper (based on the 'statewide sample') was conducted during the 1990-91 school year

**Study end date:** unclear

**Funding source:** unclear

**Conflict of interest:** unclear. The study author helped create a training manual for FAST (see p 15), but the FAST developer does not appear to have participated in this study

### Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	<p><b>Comment:</b> no information on which to base a judgement</p> <p><b>Quote:</b> "at this school, 30 to 40 children were identified as "at-risk", and then randomly assigned either to recruitment into the FAST programme or to a waiting-list comparison condition" (p 89)</p>
Allocation concealment (selection bias)	Unclear risk	<p><b>Comment:</b> no details are provided on the methods used to conceal the allocation sequence</p>
Blinding of participants and personnel (performance bias) All outcomes	High risk	<p><b>Comment:</b> the condition to which participants were assigned was known to both intervention providers and participants</p>
Blinding of outcome assessment (detection bias) (parent-reported outcomes) All outcomes	High risk	<p><b>Comment:</b> parents knew their assignment conditions</p>
Blinding of outcome assessment (detection bias) (teacher-reported outcomes) All outcomes	Unclear risk	<p><b>Comment:</b> it is not clear whether teachers were aware of the student assignment conditions</p>
Incomplete outcome data (attrition bias): high attrition outcomes	High risk	<p><b>Comment:</b> the total number of students randomly assigned was given as 30 to 40, and outcomes were based on 19 to 32 students. In addition, differential efforts were made to include families who completed at least 5 FAST sessions at outcome assessment</p> <p><b>Quote:</b> "for economy of staff time, FAST trainers did not strongly encourage the collection of post-measures from families who did not complete at least five of the eight sessions of the FAST curriculum" (p 88-89)</p>

## Billingham 1993 (Continued)

Selective reporting (reporting bias)	Unclear risk	<b>Comment:</b> there is no information in the paper on which to base a judgement. The paper does seem thoroughly reported, although for studies conducted at this time, it would not be unusual to omit measured outcomes
Other bias	High risk	<b>Recruitment bias</b>  <b>Comment:</b> students were identified by their schools as being 'at risk' for leaving school early, and then their families were approached by school personnel for a home visit by FAST staff. Presumably, not all families agreed to the home visit, and not all visited families agreed to be recruited into the study, although the paper is silent on these details

## Knox 2011

Methods	<b>Design:</b> cluster-randomised trial (clusters were 4 to 10 community blocks)
Participants	<b>Location/setting:</b> Santa Ana, CA (USA) <b>Sample size:</b> 282 immigrant Latino families <b>Mean age:</b> 9.5 years <b>Sex:</b> 52% female <b>Race/ethnicity:</b> Latino = 100%
Interventions	<b>Intervention (140 families):</b> FAST consisted of family group sessions taking place once a week for 10 weeks, with each session lasting approximately 2.5 hours. During these sessions, families are guided through planned activities including family meals, family projects (e.g. designing a family flag), communication games, group activities (e.g. FAST songs), and either support group or modified parent-child play sessions <b>Control (142 families):</b> not described
Outcomes	<b>Eligible measures (outcome domain)</b> <ol style="list-style-type: none"> <li>1. Parents' report of children's aggression using the Social Competence and Behavior Evaluation Scale (child externalising behaviours)</li> <li>2. Social Competencies, that is, self-control (child externalising behaviours)</li> <li>3. Children's Self-Report of Aggression (child externalising behaviours)</li> </ol> <b>Ineligible measures (reason)</b> <ol style="list-style-type: none"> <li>1. Medical Outcomes Study Social Support Scale (measures parent perceived social support; not an outcome specified in the protocol for this review)</li> <li>2. Perception of Community-Level Collective Self-Efficacy - Parent (measures collective self-efficacy; not an outcome specified in the protocol for this review)</li> <li>3. General well-being - Parent (single item that asks parents to rate their overall health and well-being; not in a domain specified in the protocol for this review)</li> <li>4. Social problem-solving - Child (measures child problem-solving skills, which is not in a domain specified in the protocol for this review)</li> <li>5. Perceptions of community-level self-efficacy - Child (not in a domain specified in the protocol for this review)</li> </ol> <b>Timing of outcome measurement:</b> 3 months after the end of FAST, and again at 12 months
Notes	<b>Study start date:</b> not stated

## Knox 2011 (Continued)

**Study end date:** not stated

**Funding source:** US Centers for Disease Control and Prevention

**Conflict of interest:** none noted. The FAST organisation's involvement appears to have been limited to implementation planning

### Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	<b>Quote:</b> "a computer generated random number was used to randomly assign two communities to the intervention condition and two communities to the control condition" (p 68)
Allocation concealment (selection bias)	Unclear risk	<b>Comment:</b> for community-level randomisation, no indication that attempts were made to conceal the allocation sequence is given in the report
Blinding of participants and personnel (performance bias) All outcomes	High risk	<b>Comment:</b> the condition to which participants were assigned was known to intervention providers and to participants
Blinding of outcome assessment (detection bias) (parent-reported outcomes) All outcomes	High risk	<b>Comment:</b> parents were aware of their assignment conditions
Incomplete outcome data (attrition bias): high attrition outcomes	High risk	<b>Comment:</b> across outcomes, attrition rates (based on families who agreed to participate in the study) ranged from 27% to 35%. Differential attrition was high (15 percentage points more loss in the FAST group than in the control group). Multiple imputation was used to address both attrition and item non-response
Selective reporting (reporting bias)	Unclear risk	<b>Comment:</b> there is no information in the paper on which to base a judgement. The paper does seem thoroughly reported
Other bias	High risk	<b>Recruitment bias</b>  <b>Comment:</b> communities (i.e. geographic areas consisting of 4 to 10 blocks) were randomly assigned to conditions, and then families were recruited into the study

## Kratochwill 2004

Methods	<b>Design:</b> randomised controlled trial
Participants	<b>Location/setting:</b> Northern Wisconsin (USA) <b>Sample size:</b> 100 American Indian families <b>Mean age:</b> approximately 6.75 years. Age inferred from grade level and not reported in the paper <b>Sex:</b> 57% female (intervention = 56% female, control = 58% female) <b>Race/ethnicity:</b> American Indian = 100%

**Kratochwill 2004** (Continued)

Interventions	<p><b>Intervention (50 families):</b> as implemented, a FAST cycle consisted of 8 weekly, multi-family group meetings. 7 cycles were conducted sequentially over 3 years. Sessions consisted of 15 minutes of parent-child playtime, a shared meal, other family activities, and separate child play and parent discussion, and a lottery. The activities were adapted for specific fit with the 3 participating American Indian Nations, with details provided for each (see p 366), including details of parental homework, substance abuse information sessions, and 2-year monthly follow-up sessions</p> <p><b>Control (50 families):</b> control families did not receive FAST treatment. No other details are provided</p>	
Outcomes	<p><b>Eligible measures (outcome domain)</b></p> <ol style="list-style-type: none"><li>1. Curriculum Based Measures: reading (child school performance)</li><li>2. Curriculum Based Measures: math (child school performance)</li><li>3. Child Behavior Checklist: internalising - Parent and Teacher Report (child internalising behaviours)</li><li>4. Child Behavior Checklist: externalising - Parent and Teacher Report (child externalising behaviours)</li><li>5. Family Adaptability and Cohesion Evaluation Scales: adaptability - Parent (family relationships)</li><li>6. Family Adaptability and Cohesion Evaluation Scales: cohesion - Parent (family relationships)</li></ol> <p><b>Ineligible measures (reason)</b></p> <ol style="list-style-type: none"><li>1. Mathematics computation (the protocol for this review specifies that measures of school performance must be at the course grade level and above)</li><li>2. Oral reading fluency (the protocol for this review specifies that measures of school performance must be at the course grade level and above)</li><li>3. Ecobehavioural Assessment System: academic (not standardised tests or grades at the class level or above)</li><li>4. Ecobehavioural Assessment System: task management (not an eligible domain)</li><li>5. Ecobehavioural Assessment System: competing (not an eligible domain)</li><li>6. Social Skills Rating System: academic competence scale (the protocol for this review specifies that measures of school performance must be at the course grade level and above)</li><li>7. Social Skills Rating System: social skills scale - Parent and Teacher (not an eligible domain)</li><li>8. Social Skills Rating System: problem behaviours Scale - Parent and Teacher (combines internalising and externalising behaviours and therefore not in a specific domain listed in the protocol for this review)</li><li>9. Child Behavior Checklist: social problems - Parent and Teacher (not an eligible domain)</li><li>10. Family support (not a measure of family relationships)</li></ol> <p><b>Timing of outcome measurement:</b> immediately after FAST and 9 to 12 months of follow-up</p>	
Notes	<p><b>Study start date:</b> not stated</p> <p><b>Study end date:</b> not stated</p> <p><b>Funding source:</b> US Department of Education</p> <p><b>Conflict of interest:</b> the FAST developer was one of the co-authors on this paper</p>	
<b>Risk of bias</b>		
<b>Bias</b>	<b>Authors' judgement</b>	<b>Support for judgement</b>
Random sequence generation (selection bias)	Unclear risk	<p><b>Comment:</b> no details about the randomisation procedure are provided</p> <p><b>Quote:</b> "within each cycle, all children were matched on the basis of a number of designated characteristics and then randomly assigned either to participate in the FAST programme or to serve as non-FAST controls" (p 364)</p>
Allocation concealment (selection bias)	Unclear risk	<p><b>Comment:</b> no details are provided on methods used to conceal the allocation sequence</p>

**Kratochwill 2004** (Continued)

**Quote:** "parents and children who participated in the research project (N = 100 families) were openly and universally recruited. Families were recruited at a school event and by sending information about the project home with all students in grades K-2 in the participating schools" (p 363)

Blinding of participants and personnel (performance bias) All outcomes	High risk	<b>Comment:</b> the condition to which participants were assigned was known to both intervention providers and participants
Blinding of outcome assessment (objective outcomes) All outcomes	Low risk	<b>Comment:</b> this outcome is based on standardised tests
Blinding of outcome assessment (detection bias) (parent-reported outcomes) All outcomes	High risk	<b>Comment:</b> parents knew the conditions to which they were assigned
Blinding of outcome assessment (detection bias) (teacher-reported outcomes) All outcomes	Unclear risk	<b>Comment:</b> the trialists attempted to keep teachers blind to assignment condition, but no information is available on how successful these efforts were  <b>Quote:</b> "to the best of the researchers' ability, all teachers, observers, and testers were kept 'blind' concerning participants' condition status" (p 364)
Incomplete outcome data (attrition bias): high attrition outcomes	High risk	<b>Comment:</b> after randomising matched pairs of students to conditions, the trialists dropped incomplete pairs from analysis. For teacher-reported outcomes, analyses are based on 49 or 50 pairs of families. For parent-reported outcomes, analyses are based on 19 to 24 pairs of families. Observation-based measures are based on 40 pairs of families
Selective reporting (reporting bias)	Low risk	<b>Comment:</b> we obtained the printouts of the statistical analyses, suggesting it is likely that all measured outcomes are available for this review
Other bias	High risk	<b>Recruitment bias</b>  <b>Comment:</b> families were recruited after schools were randomly assigned to conditions  <b>Quote:</b> "parents and children who participated in the research project (N = 100 families) were openly and universally recruited. Families were recruited at a school event and by sending information about the project home with all students in grades K-2 in the participating schools" (p 363)  <b>Loss of clusters</b>  <b>Comment:</b> no clusters were lost from the analysis

**Kratochwill 2009**

Methods	<b>Design:</b> randomised controlled trial
Participants	<b>Location/setting:</b> Madison, WI (USA)  <b>Sample size:</b> 134 children (half of whom were identified as at risk for special education services) and their families



**Kratochwill 2009** (Continued)

**Mean age:** approximately 6.4 years. Age inferred from grade level and not reported in the paper

**Sex:** 57.5% female (intervention = 56.7% female, control = 58.2% female)

**Race/ethnicity:** Asian = 13%, African American = 35%, Latino = 12%, White = 40%

Interventions	<p><b>Intervention (67 children):</b> as implemented, a FAST cycle consisted of 8 weekly, multi-family group after-school meetings. 8 standardised cycles (1 per school) were conducted over a 3-year period. The sessions were structured and included interactive and experiential learning, a 15-minute parent-child playtime, meals, and songs</p> <p><b>Control (67 children):</b> "services as usual" (quote, p 250)</p>
Outcomes	<p><b>Eligible measures (outcome domain)</b></p> <ol style="list-style-type: none"> <li>1. Child Behavior Checklist: internalising - Parent and Teacher Report (child internalising behaviour)</li> <li>2. Child Behavior Checklist: externalising - Parent and Teacher Report (child externalising behaviour)</li> <li>3. Family Adaptability and Cohesion Evaluation Scales: adaptability - Parent (family relationships)</li> <li>4. Family Adaptability and Cohesion Evaluation Scales: cohesion - Parent (family relationships)</li> <li>5. Math and reading achievement tests (child school performance). Note that the exact nature of these tests is unclear but may be related to those used in <a href="#">Kratochwill 2004</a></li> </ol> <p><b>Ineligible measures (reason)</b></p> <ol style="list-style-type: none"> <li>1. Social Skills Rating System: academic competence scale (the protocol for this review requires that measures of academic achievement must be at the course grade level and above)</li> <li>2. Social Skills Rating System: social skills scale - Parent and Teacher (not an eligible domain)</li> <li>3. Social Skills Rating System: problem behaviour - Parent and Teacher (combines internalising and externalising behaviours)</li> <li>4. Child Behavior Checklist: social problems - Parent and Teacher (not an eligible domain)</li> <li>5. Child Behavior Checklist: thought problems - Parent and Teacher (not an eligible domain)</li> <li>6. Child Behavior Checklist: withdrawn - Parent and Teacher (ineligible because it is included in the 'internalising' composite measure)</li> <li>7. Child Behavior Checklist: somatic complaints - Parent and Teacher (ineligible because it is included in the 'internalising' composite measure)</li> <li>8. Child Behavior Checklist: anxious/depressed - Parent and Teacher (ineligible because it is included in the 'internalising' composite measure)</li> <li>9. Child Behavior Checklist: delinquent behaviour - Parent and Teacher (ineligible because it is included in the 'externalising' composite measure)</li> <li>10. Child Behavior Checklist: aggressive behaviour - Parent and Teacher (ineligible because it is included in the 'externalising' composite measure)</li> <li>11. Family Support Scale ("measures availability and helpfulness of social support for the family" (quote, p 253) and therefore not a measure of family relationships)</li> </ol> <p><b>Timing of outcome measurement:</b> immediately after completion of FAST and 9 to 12 months of follow-up</p>
Notes	<p><b>Study start date:</b> perhaps implemented in the 2000-01 school year (see p 260)</p> <p><b>Study end date:</b> perhaps spring 2002</p> <p><b>Funding source:</b> none mentioned</p> <p><b>Conflict of interest:</b> the FAST developer was one of the co-authors on this paper</p>
<b>Risk of bias</b>	
<b>Bias</b>	<b>Authors' judgement      Support for judgement</b>

**Kratochwill 2009** (Continued)

Random sequence generation (selection bias)	Unclear risk	<p><b>Comment:</b> no details about the randomisation procedure are provided</p> <p><b>Quote:</b> "(1) as many volunteering students as possible were matched on the basis of a number of designated characteristics, and then (2) randomly assigned either to participate in the FAST programme or to serve as non-FAST controls. This process produced a total of 67 matched pairs" (p 250)</p>
Allocation concealment (selection bias)	Unclear risk	<p><b>Comment:</b> no details are provided on methods used to conceal the allocation sequence</p> <p><b>Quote:</b> "(1) as many volunteering students as possible were matched on the basis of a number of designated characteristics, and then (2) randomly assigned either to participate in the FAST programme or to serve as non-FAST controls. This process produced a total of 67 matched pairs" (p 250)</p>
Blinding of participants and personnel (performance bias) All outcomes	High risk	<p><b>Comment:</b> the condition to which participants were assigned was known to both intervention providers and participants</p>
Blinding of outcome assessment (objective outcomes) All outcomes	Low risk	<p><b>Comment:</b> this outcome is based on standardised tests that are routinely administered to all students</p>
Blinding of outcome assessment (detection bias) (parent-reported outcomes) All outcomes	High risk	<p><b>Comment:</b> parents knew the conditions to which they were assigned</p>
Blinding of outcome assessment (detection bias) (teacher-reported outcomes) All outcomes	Unclear risk	<p><b>Comment:</b> the triallists attempted to keep teachers blind to assignment condition, but no information is available on how successful these efforts were</p> <p><b>Quote:</b> "to the best of the researchers' ability, all teachers, observers, and testers were kept 'blind' concerning participants' condition status" (p 364)</p>
Incomplete outcome data (attrition bias): high attrition outcomes	High risk	<p><b>Comment:</b> the triallists state that 225 families were approached for participation, that 172 agreed to participate, and that 134 were matched and potentially included in outcome analyses. Specifically, after randomising matched pairs of students to conditions, the triallists dropped incomplete pairs from the analysis. For teacher-reported outcomes at the immediate post-test, analyses are based on 60 pairs of families. For parent-reported outcomes, analyses are based on 53 pairs of families</p> <p>For teacher-reported outcomes at the follow-up post-test, analyses are based on 39 pairs of families. For parent-reported outcomes, analyses are based on 31 pairs of families</p>
Selective reporting (reporting bias)	Low risk	<p><b>Comment:</b> we obtained the printouts of the statistical analyses, suggesting it is likely that all measured outcomes are available for this review</p>
Other bias	High risk	<p><b>Recruitment bias</b></p> <p><b>Comment:</b> the triallists state that 225 families were approached for participation and that outcome analyses were based on, at most, 60 pairs of students. It is unknown whether 225 represents the totality of eligible families in the 3 schools</p>

## Layzer 2001

Methods	<b>Design:</b> randomised controlled trial
Participants	<p><b>Location/setting:</b> New Orleans, LA (USA)</p> <p><b>Sample size:</b> 407 largely (90%) African American children and their families</p> <p><b>Mean age:</b> 7.52 years (intervention = 7.37, control = 7.67)</p> <p><b>Sex:</b> 38% female (intervention = 38% female, control = 39% female)</p> <p><b>Race/ethnicity:</b> African American = 90%. The race/ethnicity of the remaining 10% of children was not specified</p>
Interventions	<p><b>Intervention (207 children):</b> as implemented, FAST consisted of 3-hour sessions held over 8 consecutive weeks. Sessions consisted of 15 minutes of parent-child time, a shared family meal, other family activities, and separate child play and parent discussion. The trialists did not provide detail on the content of the parent discussions, but these appeared to have both a parent self-help group component and an alcohol/drug abuse education component. Some families included in the evaluation also attended FASTWORKS, a series of parent-organised monthly support meetings that are intended to continue and extend the social network established during FAST</p> <p><b>Control (200 children):</b> weekly (over 8 weeks) receipt of a commercial pamphlet on parenting</p>
Outcomes	<p><b>Eligible measures (outcome domain)</b></p> <ol style="list-style-type: none"> <li>1. Average grade in all subjects (child school performance)</li> <li>2. Child Behavior Checklist: internalising - Parent and Teacher Report (child internalising behaviours)</li> <li>3. Child Behavior Checklist: externalising - Parent and Teacher Report (child externalising behaviours)</li> <li>4. % of days absent (school year) (child school attendance)</li> <li>5. Proportion of parents who visited school (parent engagement in education)</li> <li>6. Proportion of parents who participated in school activities (parent engagement in education)</li> <li>7. Hours per month of community involvement (parent involvement in community-based activities)</li> <li>8. Proportion of parents who volunteer (parent involvement in community-based activities)</li> <li>9. Average hours of volunteer work per week (parent involvement in community-based activities)</li> <li>10. Proportion of parents who have held higher-level leadership positions (parent involvement in community-based activities)</li> <li>11. Proportion of parents who have held lower-level leadership positions (parent involvement in community-based activities)</li> <li>12. Family Environment Scale: cohesiveness, expressiveness, conflict, independence, organisation, and control subscales (family relationships)</li> </ol> <p><b>Ineligible measures (reason)</b></p> <ol style="list-style-type: none"> <li>1. Child Social Activities (not in a domain specified in the protocol for this review)</li> <li>2. Social Skills Rating System: total (assesses prosocial behaviour, not clearly internalising or externalising, and therefore not in a domain specified in the protocol for this review)</li> <li>3. Social Skills Rating System: academic competence scale - Teacher (teacher-rated academic competence is not a measure of school performance, and therefore is not in a domain specified in the protocol for this review)</li> <li>4. Student Evaluation - Teacher (does not rise to the level of a standardised measure of behaviour as required by the protocol for this review)</li> <li>5. Behaviour Grade (does not rise to the level of a standardised measure of behaviour as required by the protocol for this review)</li> <li>6. % of days absent for final quarter (could be a measure of child school attendance but the study provides an attendance measure for the entire school year)</li> </ol>

## Layzer 2001 (Continued)

7. Has excellent grades - Teacher (the protocol requires that measures of school performance be "grades or marks that students earn, standardised educational tests, performance tests or other objective measure of educational attainment")
8. Has grades consistent with potential - Teacher (the protocol requires that measures of school performance be "grades or marks that students earn, standardised educational tests, performance tests or other objective measure of educational attainment")
9. Family Routines Questionnaire ("assesses the stability or consistency of shared family activities" (quote, p B2-26); not in a domain specified in the protocol for this review)
10. Parent as a Teacher (assessed for parents with preschool children only, assesses "parents' feelings about their child's need for creativity and play, about their own role as teacher of their child, and about their level of patience with their child" (quote, B2-26); not in a domain specified in the protocol for this review)
11. Parent strategies for resolving parent/child conflict (could be family relationships, but unclear what constitutes a 'good' response)
12. Parent number of social activities in past month (not in a domain specified in the protocol for this review)
13. Proportion of parents who felt lonely in past month (not in a domain specified in the protocol for this review)
14. Proportion of neighbourhood resources used by family (not in a domain specified in the protocol for this review)
15. Proportion of parents contacted by school for positive reasons (includes both academic and behaviour reasons and therefore mixes domains)
16. Proportion of parents contacted by school for negative reasons (includes both academic and behaviour reasons and therefore mixes domains)

**Timing of outcome measurement:** 1-year follow-up

Notes	<p><b>Study start date:</b> September 1997</p> <p><b>Study end date:</b> Spring 1999</p> <p><b>Funding source:</b> US Department of Health and Human Services</p> <p><b>Conflict of interest:</b> none noted. The evaluation was conducted by an independent research firm. The FAST organisation was involved in implementation planning</p>
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### Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	<p><b>Comment:</b> no details about randomisation are given in the report</p> <p><b>Quote:</b> "randomly assigned to FAST or to the alternative treatment by Abt Associates staff" (p B2-5)</p>
Allocation concealment (selection bias)	Unclear risk	<p><b>Comment:</b> because no details about randomisation were given in the report, it is not clear if the investigators could have foreseen assignment conditions when randomisation occurred</p>
Blinding of participants and personnel (performance bias) All outcomes	High risk	<p><b>Comment:</b> the condition to which participants were assigned was known to both intervention providers and participants</p> <p><b>Quote:</b> "the families assigned to FAST were visited by the FAST coordinator and a parent liaison and asked to join FAST; in addition, these families were asked if they would be part of our study. The families assigned to the alternative treatment were visited by an Abt Associates field staff member and asked to join the alternative treatment and to be in the study" (p B2-5)</p>

## Layzer 2001 (Continued)

Blinding of outcome assessment (detection bias) (parent-reported outcomes) All outcomes	High risk	<b>Comment:</b> parents knew the conditions to which they were assigned
Blinding of outcome assessment (detection bias) (teacher-reported outcomes) All outcomes	Unclear risk	<b>Comment:</b> students were referred for participation in the study by teachers, but it is unclear whether the trialists kept teachers blind to the conditions to which the students were assigned
Incomplete outcome data (attrition bias): low attrition outcomes	Low risk	<b>Comment:</b> these outcomes all experienced low attrition (5% to 6%): parent-reported internalising and externalising behaviour, parent-reported family relations, parent-reported involvement in the community and in school
Incomplete outcome data (attrition bias): high attrition outcomes	High risk	<b>Comment:</b> teacher-reported internalising and externalising behaviour, grades, and absences all experienced high levels of attrition (24% to 34% of the sample)
Selective reporting (reporting bias)	Unclear risk	<b>Comment:</b> there is no information in the paper on which to base a judgement. The paper does seem thoroughly reported, although for studies conducted at this time, it would not be unusual to omit measured outcomes. The trialists do note that a pilot test was implemented and analyses were conducted at a single site in Wisconsin (USA), but results were not made available (see p B24-5)
Other bias	High risk	<b>Recruitment bias</b>  <b>Comment:</b> it is clear that participants and program staff knew the assignment condition before obtaining consent, leaving open the possibility that this knowledge may have affected the behaviour of the program staff or the participants or both. In addition, it is unclear whether all eligible families in the randomly assigned schools were approached to participate  <b>Quote:</b> "the families assigned to FAST were visited by the FAST coordinator and a parent liaison and asked to join FAST; in addition, these families were asked if they would be part of our study. The families assigned to the alternative treatment were visited by an Abt Associates field staff member and asked to join the alternative treatment and to be in the study" (p B2-5)

## Lord 2018

Methods	<b>Design:</b> cluster-randomised trial, with stratification by region
Participants	<b>Location/setting:</b> various communities in England (UK)  <b>Sample size:</b> 158 schools  <b>Mean age:</b> 5.5 years. Age not provided but inferred for the overall sample from the fact that these are Year 1 students  <b>Sex:</b> not provided  <b>Race/ethnicity:</b> not provided
Interventions	<b>Intervention (79 schools):</b> "groups of parents and their children (usually around 5–8 families in each group) attend eight weekly 2.5-hour group sessions after school, delivered by trained local part-

## Lord 2018 (Continued)

ners...Sessions are designed to encourage good home routines around mealtimes, bedtimes and homework" (quote, p 4)

Each session followed the same plan and included family activities, a shared family meal, separate parent and child group work, and parent-child play time. Some families included in the evaluation also attended FASTWORKS, a series of parent-organised monthly support meetings that are intended to continue and extend the social network established during FAST

**Control (79 schools):** no information is provided about the experiences of control families, except that they did not receive the FAST intervention

Outcomes	<b>Eligible measures (outcome domain)</b>  1. Key Stage 1: reading and arithmetic (child school performance)  <b>Ineligible measures (reason)</b>  1. Strengths and Difficulties Questionnaire: total difficulties scale (comprises 4 subscales from the Strengths and Difficulties Questionnaire: Emotional Symptoms; Conduct Problems; Hyperactivity/Inattention; Peer Relationship Problems. In the protocol for this review, Emotional Symptoms are clearly in the internalising domain, and Conduct Problems are clearly in the externalising domain. Therefore, the Total Difficulties Scale has elements from 2 distinct domains and does not fit into a single domain specified in the protocol for this review 2. Strengths and Difficulties Questionnaire: impact supplement (has aspects that address internalising symptoms and externalising symptoms: "teachers were asked if they thought pupils were facing behaviour or emotional problems and, if so, enquired further about chronicity, distress, social impairment, and burden to others" (quote, p 42). Because the outcome has elements from 2 distinct domains, it does not fit into a single domain specified in the protocol for this review 3. Strengths and Difficulties Questionnaire: prosocial behaviour (not in a domain specified in the protocol for this review)  <b>Timing of outcome measurement:</b> approximately 15 to 18 months after FAST participation	
Notes	<b>Study start date:</b> mid-2015  <b>Study end date:</b> mid-2017  <b>Funding source:</b> Education Endowment Foundation  <b>Conflict of interest:</b> none noted. The evaluation was conducted by an independent research firm. Implementation support was provided by Save the Children (UK), via a licence from the FAST organisation	
<b>Risk of bias</b>		
<b>Bias</b>	<b>Authors' judgement</b>	<b>Support for judgement</b>
Random sequence generation (selection bias)	Low risk	<b>Comment:</b> statistical software (SPSS) was used to generate the random sequence <b>Quote:</b> "randomisation was carried out by an NFER statistician using a full audit trail in SPSS (see Appendix G for the syntax used)" (p 20)
Allocation concealment (selection bias)	Low risk	<b>Comment:</b> given that randomisation was carried out using statistical software, the allocation sequence should not have been predictable in advance of enrolment
Blinding of participants and personnel (performance bias) All outcomes	High risk	<b>Comment:</b> all participants and personnel knew the conditions to which participants were assigned



## Lord 2018 (Continued)

Blinding of outcome assessment (objective outcomes) All outcomes	Low risk	<p><b>Comment:</b> child academic achievement was derived from state mandated examinations scored by teachers. While the teachers likely knew children's assignment conditions, it does not seem likely that this knowledge would have influenced their scoring</p> <p><b>Quote:</b> "for bias to occur, a teacher would have to (advertently or inadvertently) assist a child/children in a FAST school differentially to a child in a control school during a statutory national test. However, we considered this a highly unlikely scenario. Testing took place a whole year or more after the eight-week FAST delivery, most likely with a different class teacher, and hence teachers were unlikely to link conducting this test with the intervention. Key Stage 1 testing is statutory, and we expected schools to follow their normal procedures" (p 17)</p>
Incomplete outcome data (attrition bias): high attrition outcomes	High risk	<p><b>Comment:</b> this trial employed randomisation at the school level. 35% of FAST schools and 19% of control schools dropped out of the trial. The study authors concluded that attrition likely represented an important source of bias in their study. All students were considered part of the sample to which their schools were assigned, but data for some students were missing (even after school-level attrition was accounted for). In total, 50% of FAST students and 33% of control students did not provide outcome data</p> <p><b>Quote:</b> baseline analyses were "consistent with the idea that lower performing schools dropped out and that this kind of attrition was greater in the intervention group" (p 35)</p>
Selective reporting (reporting bias)	Low risk	<p><b>Comment:</b> the outcomes were specified in a preregistered protocol</p> <p><b>Quote:</b> "the FAST trial is registered at <a href="https://doi.org/10.1186/ISRCT-N53386443">https://doi.org/10.1186/ISRCT-N53386443</a>" (p 14)</p>
Other bias	Low risk	<p><b>Recruitment bias</b></p> <p><b>Comment:</b> schools were assigned to conditions before parents were recruited into the programme. Participants and programme staff knew the assignment conditions before obtaining consent, leaving open the possibility that this knowledge may have influenced the behaviour of programme staff or participants, or both. However, child outcomes were intended to be collected for all children in FAST schools</p> <p><b>Quote:</b> "all parents of pupils who were in Year 1 in the academic year 2015/2016 were eligible to take part in the FAST trial" (p 16), with FAST staff and parents recruiting families into the programme</p>

## López Turley 2017

Methods	<b>Design:</b> cluster-randomised trial (clusters were schools)
Participants	<p><b>Location/setting:</b> San Antonio, TX, and Phoenix, AZ (USA)</p> <p><b>Sample size:</b> 3084 largely racial/ethnic minority students and their families</p> <p><b>Mean age:</b> approximately 6.5 years. Age not provided in the various papers reporting on this study, but inferred from the grade level of the children</p> <p><b>Sex:</b> not provided</p> <p><b>Race/ethnicity:</b> Latino = approximately 74%, African American = 9%. The race/ethnicity of the remaining students was not specified</p>
Interventions	<b>Intervention (approximately 1400* children):</b> as implemented, FAST consisted of 8 weekly group meetings involving a shared meal (each week, 1 family won a gift card from a grocery store, and the fol-

**López Turley 2017** (Continued)

lowing week that family bought or prepared the group meal; children participated in the set-up before and cleaned up after the meal); family games; and other activities that encourage taking turns, sharing feelings, and delaying gratification

**Control (approximately 1400\* children):** described as business as usual

\*Analyses based on 2832 children

Outcomes	<b>Eligible measures (outcome domain)</b>  1. Strengths and Difficulties Questionnaire: emotional symptoms and peer problems subscales - Teacher (child internalising behaviours) 2. Strengths and Difficulties Questionnaire: conduct problems and hyperactivity subscales - Teacher (child externalising behaviours)  <b>Ineligible measures (reason)</b>  1. Parent social capital (assessed by asking parents about the number of other parents from the school that they know, the extent of shared expectations and trust among parents at the school, and the social support that they give and receive from other parents at the school; none of these fit into a domain specified in the protocol for this review)  <b>Timing of outcome measurement:</b> immediately after the FAST intervention	
Notes	<b>Study start date:</b> probably 2008 (Gamoran 2012)  <b>Study end date:</b> probably 2010  <b>Funding source:</b> US National Institute of Child Health and Human Development  <b>Conflict of interest:</b> none noted. The involvement of the FAST organisation is unclear. The paper does indicate that FAST was delivered by trained parents and professionals (p 209) but does not indicate the source of the training	
<b>Risk of bias</b>		
<b>Bias</b>	<b>Authors' judgement</b>	<b>Support for judgement</b>
Random sequence generation (selection bias)	Unclear risk	<b>Comment:</b> the method used to generate the allocation sequence is not described in the paper  <b>Quote:</b> "schools were randomly assigned to one of the two cohorts, and then they were randomly assigned to treatment and control groups" (p 6)
Allocation concealment (selection bias)	High risk	<b>Comment:</b> it is unclear whether school assignment conditions could have been foreseen in advance of or during enrolment. However, it is clear that participants and programme staff knew the assignment conditions before obtaining consent, leaving open the possibility that this knowledge may have influenced the behaviour of programme staff or participants, or both  <b>Quote:</b> "all potential participants learned about the study, chose whether to consent to be in the study...Parents in the treatment schools also learned about FAST and chose whether to consent. Teachers also learned about the study, chose whether to participate" (p 7)
Blinding of participants and personnel (performance bias) All outcomes	High risk	<b>Comment:</b> all participants and personnel knew the condition to which participants were assigned

## López Turley 2017 (Continued)

Blinding of outcome assessment (detection bias) (teacher-reported outcomes) All outcomes	High risk	<b>Comment:</b> the study authors collected outcome information on students of teachers who, due to school-level randomisation, appeared to have been aware of assignment conditions
Incomplete outcome data (attrition bias): low attrition outcomes	Low risk	<b>Comment:</b> the trialists attempted to collect teacher reports of internalising and externalising behaviour for all students assigned to conditions
Selective reporting (reporting bias)	High risk	<b>Comment:</b> the report does not provide parent-reported outcomes due to "high levels of non-random missingness for the parent outcomes data in the full sample" (quote)
Other bias	High risk	<p><b>Recruitment bias</b></p> <p><b>Comment:</b> schools were assigned to conditions before parents were recruited into the programme. Participants and programme staff knew the assignment conditions before obtaining consent, leaving open the possibility that this knowledge may have influenced the behaviour of programme staff or participants, or both</p> <p><b>Quote:</b> "The research team and staff from the local social service agencies recruited families to the study at both FAST and comparison schools ... Potential participants learned about the study, chose whether to consent to the study, and received a \$10 gift card as compensation for filling out a short 3-page pre-test questionnaire. Parents in the FAST schools also learned about FAST and chose whether to consent and participate in FAST" (p 210)</p>

## McDonald 2012b

Methods	<b>Design:</b> randomised controlled trial
Participants	<p><b>Location/setting:</b> Madison, WI (USA)</p> <p><b>Sample size:</b> 38 Hmong families</p> <p><b>Mean age:</b> 8.6 years</p> <p><b>Gender:</b> 43% female</p> <p><b>Race/ethnicity:</b> Hmong = 100%</p>
Interventions	<p><b>Intervention (analyses based on 5 children and their families):</b> FAST, as implemented in this study, was described as follows. The intervention was hosted by the community refugee centre; researchers requested that 1 parent attend with up to 3 children due to space limitations – although eventually, more and more family members attended; unlike many other FAST implementations, communal meals occurred at completion of the session because groups arrived at 9 am on Saturdays. The core components/processes used were team membership, parental compliance with requests during meals, coaching of parents, music, parent groups, children's activity, and responsive play. Furthermore, study authors worked with Hmong elders to develop cultural adaptations for FAST</p> <p><b>Control (analyses based on 5 children and their families):</b> wait-list control</p>
Outcomes	<p><b>Eligible measures (outcome domain)</b></p> <p>1. Child Behavior Checklist: internalising (child internalising behaviours)</p>

## McDonald 2012b (Continued)

2. Child Aggressiveness: no specific information on how this was measured but likely was measured using the externalising subscale of the Child Behavior Checklist (child externalising behaviours)
3. Family Adaptation and Cohesion Scales II (FACES II; family relationships)
4. Family Cohesion: no specific information on how this was measured, but likely was measured using the cohesion subscale of FACES II (family relationships)

### Ineligible measures (reason)

1. Social Skills Rating System: child social skills (not in a domain listed in the protocol for this review)

**Timing of outcome measurement:** immediately after completion of FAST intervention

Notes	<b>Study start date:</b> not stated	
	<b>Study end date:</b> not stated	
	<b>Funding source:</b> none mentioned	
	<b>Conflict of interest:</b> the FAST developer was one of the co-authors on this paper	
<b>Risk of bias</b>		
<b>Bias</b>	<b>Authors' judgement</b>	<b>Support for judgement</b>
Random sequence generation (selection bias)	Unclear risk	<b>Comment:</b> no information is given in the study about the method used to assign families to conditions  <b>Quote:</b> "randomly assigned to 'FAST now' or 'FAST later'" (p 118)
Allocation concealment (selection bias)	Unclear risk	<b>Comment:</b> because no details about randomisation were given in the report, it is not clear whether investigators could have foreseen assignment conditions when randomisation occurred
Blinding of participants and personnel (performance bias) All outcomes	High risk	<b>Comment:</b> the condition to which participants were assigned was known to both intervention providers and participants
Blinding of outcome assessment (detection bias) (parent-reported outcomes) All outcomes	High risk	<b>Comment:</b> parents knew the conditions to which they were assigned
Incomplete outcome data (attrition bias): high attrition outcomes	High risk	<b>Comment:</b> the exact number of families randomised to conditions cannot be determined from the report. Outcome data are based on families from Cohort 1 who attended at least 1 FAST session and provided outcome data (10 of 38 families)
Selective reporting (reporting bias)	Unclear risk	<b>Comment:</b> there is no information in the paper on which to base a judgement. The paper does seem thoroughly reported, although for studies conducted at this time, it would not be unusual to omit measured outcomes
Other bias	Low risk	<b>Comment:</b> no other problems were identified

**Moberg 2007**

Methods	<b>Design:</b> cluster-randomised controlled trial (classrooms within schools were randomly assigned to conditions)
Participants	<p><b>Location/setting:</b> Milwaukee, WI (USA)</p> <p><b>Sample size:</b> at baseline, 473 largely racial/ethnic minority students (African American and Latino) and their families</p> <p><b>Mean age:</b> intervention = 7.9 years, control = 7.7 years</p> <p><b>Sex:</b> intervention = 53% female, control = 61% female</p> <p><b>Race/ethnicity:</b> not provided</p>
Interventions	<p><b>Intervention (272 students at baseline, approximately 216 at follow-up):</b> as implemented, FAST involved the following: "structured family activity meetings were held for 8 consecutive weeks. Each FAST meeting, held during the evening in the school, was approximately 2 1/2 hours in length ... All members of the family were invited to participate as a family unit, with child and infant care provided as needed during the session. At least four trained facilitators, one on staff at the school (usually a guidance counselor or social worker), two community agency professionals, and a parent of a child in the school, led the sessions. Each weekly meeting included the following interactive units: 1) family communication activities (Feelings Charade; Scribbles) and shared meal (one hour); 2) adult dyads - spouse or other adult communication (15 minutes); 3) adult self-help group (45 minutes); 4) children's peer-connecting group activity (1 hour); 5) parent and child quality time together using non-directed - special play (15 minutes); 6) full group assembly for lottery prizes, personal achievement announcements, and a goodbye ritual" (quote, p 9)</p> <p><b>Control (201 students at baseline, approximately 143 at follow-up):</b> FAME, which involved "weekly provision of parenting skills booklets which were mailed to the home during the same 8 weeks that FAST was conducted, along with an invitation to a lecture on parenting and family life" (quote, p 9)</p>
Outcomes	<p><b>Eligible measures (outcome domain)</b></p> <ol style="list-style-type: none"> <li>1. Standardised test scores for reading, math, and science (child school performance)</li> <li>2. Child Behavior Checklist: internalising - Parent and Teacher (child internalising behaviours)</li> <li>3. Child Behavior Checklist: externalising - Parent and Teacher (child externalising behaviours)</li> <li>4. Parent-School-Community Involvement Survey (parental engagement with education)</li> <li>5. Family Attachment and Changeability Index (family relationships)</li> </ol> <p><b>Ineligible measures (reason)</b></p> <ol style="list-style-type: none"> <li>1. Family Social Support (items tap perception of being supported by the community; does not fit into a domain specified in the protocol for this review)</li> <li>2. Generalized Expectancy of Success (items tap "belief that in most situations one is able to attain desired goals" (quote, p 11); while "be[ing] a good parent" (quote, p 11) is an item on the 30-item scale; it is not a measure of parental self-efficacy as required by the protocol for this review)</li> <li>3. Child Behavior Checklist: academic performance scale - Teacher (teacher report of child academic performance, but protocol for this review requires objective measures like grades or standardised test scores)</li> <li>4. Social Skills Rating System: academic competence scale - Teacher (measure "asks teachers to compare the child to her classroom peers on skills in reading, mathematics, and motivation to succeed academically" (quote, p 12); does not fit into a domain specified by the protocol for this review)</li> <li>5. Social Skills Rating System: child social skills scores (composite of the following subscales from the Social Skills Rating System: cooperation, assertion, responsibility, and self-control; does not fit into a domain specified in the protocol for this review)</li> </ol> <p><b>Timing of outcome measurement:</b> immediately after FAST with 12-month and 24-month follow-up</p>
Notes	<b>Study start date:</b> not stated

**Moberg 2007** (Continued)

**Study end date:** not stated

**Funding source:** US National Institute on Drug Abuse

**Conflicts of interest:** the FAST developer is a co-author of this paper and authored a second paper describing results for this sample ([McDonald 2006](#))

**Risk of bias**

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	<b>Comment:</b> this is a cluster-randomised trial. Clusters were assigned to conditions via a coin toss  <b>Quote:</b> "randomly assigned (via a coin toss) ... to either FAST or FAME during each of two semesters" (p 8)
Allocation concealment (selection bias)	Low risk	<b>Comment:</b> under the assumption that the coin tossing procedure was fair (e.g. there were rules regarding re-flipping), then foreknowledge of the next assignment likely was not possible
Blinding of participants and personnel (performance bias) All outcomes	High risk	<b>Comment:</b> the condition to which participants were assigned was known to both intervention providers and participants
Blinding of outcome assessment (detection bias) (parent-reported outcomes) All outcomes	High risk	<b>Comment:</b> parents knew their assignment conditions
Blinding of outcome assessment (detection bias) (teacher-reported outcomes) All outcomes	Unclear risk	<b>Comment:</b> the paper provides conflicting information about whether teachers were consistently blind to conditions <b>Quote:</b> "teachers ...were blind to condition" (p 28) <b>Quote:</b> "teachers...in general were blind to the child's assignment to condition" (p 48)
Incomplete outcome data (attrition bias): low attrition outcomes	Low risk	<b>Comment:</b> attrition was low (2% to 3%) for the following outcomes, all measured immediately after completion of the intervention and at 1-year follow-up: family attachment, parent involvement with school, internalising behaviour (parent rating), and externalising behaviour (parent teacher rating), with a mean difference in attrition rates of about 1 percentage point
Incomplete outcome data (attrition bias): high attrition outcomes	High risk	<b>Comment:</b> attrition was high (16% to 37%) for the following outcomes, all measured 2 years after completion of the intervention: family attachment, parent involvement with school, internalising behaviour (parent and teacher ratings), and externalising behaviour (parent and teacher ratings), with a mean difference in attrition rates of about 9 percentage points
Selective reporting (reporting bias)	Unclear risk	<b>Comment:</b> there is no information in the paper on which to base a judgement. The paper does seem thoroughly reported, although for studies conducted at this time, it would not be unusual to omit measured outcomes
Other bias	High risk	<b>Recruitment bias</b>  <b>Comment:</b> because no details about randomisation were given in the report, it is not clear whether investigators could have foreseen assignment conditions when randomisation occurred. However, it is clear that participants and programme staff knew the assignment conditions before obtaining consent,



**Moberg 2007** (Continued)

leaving open the possibility that this knowledge may have influenced the behaviour of programme staff or participants, or both

**Quote:** "it was a given ... that the families would be recruited from randomly assigned schools or classrooms, rather than assigning families at random once they had agreed to participate. The recruitment process itself is considered part of the FAST intervention, and we did not want to interfere with that aspect of the program" (p 7)

AODA: Accessibility for Ontarians With Disabilities Act.

FAME: Family Education programme.

FAST: Families and Schools Together.

NFER: National Foundation for Educational Research.

RCT: randomised controlled trial.

SPSS: Statistical Package for the Social Sciences.

**Characteristics of excluded studies** [ordered by study ID]

Study	Reason for exclusion
Ackley 2010	Not an RCT
Blechman 1981	Intervention was not FAST
Coie 1998	Intervention was not FAST
Crozier 2010	Not an RCT
Fischer 2003	Not an RCT
Fuchs 2008	Not an RCT
Hernandez 2000	Not an RCT
Maalouf 2014	Not an RCT
McDonald 1997	Not an empirical study
McDonald 1998	Not an RCT
McDonald 2009b	Not an RCT
McDonald 2012a	Not an empirical study
McDonald 2016	Not an empirical study
Patrikakou 2005	Not an empirical study
Sass 1999	Not an RCT
Spoth 2008	Intervention was not FAST
Warren 2005	Not an empirical study
Wattenberg 1996	Not an empirical study

FAST: Families and Schools Together.

RCT: randomised controlled trial.

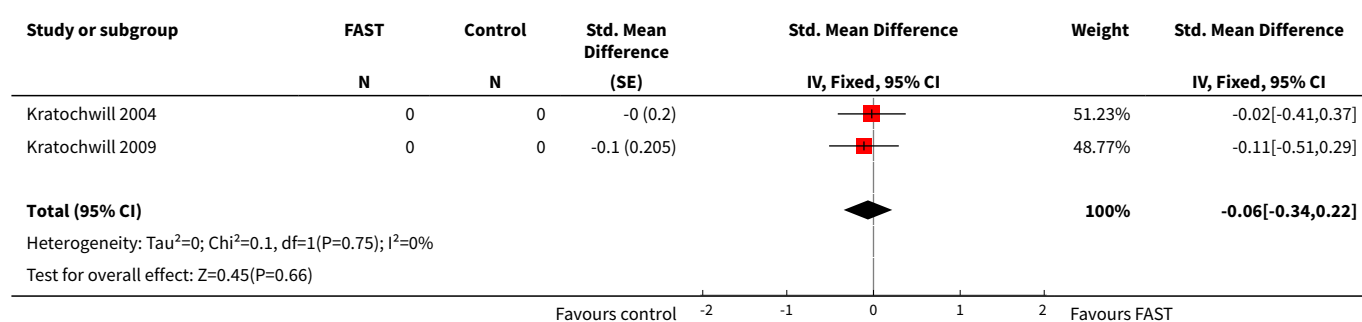
Note: the studies identified in this table were selected for full-text screening but were determined not eligible for review. We selected studies for full-text screening if, on the basis of information in the titles and abstracts, they could not be definitively ruled ineligible. We have not listed in these tables studies screened at full text that were duplicates of, or supplementary reports to, an eligible study.

## DATA AND ANALYSES

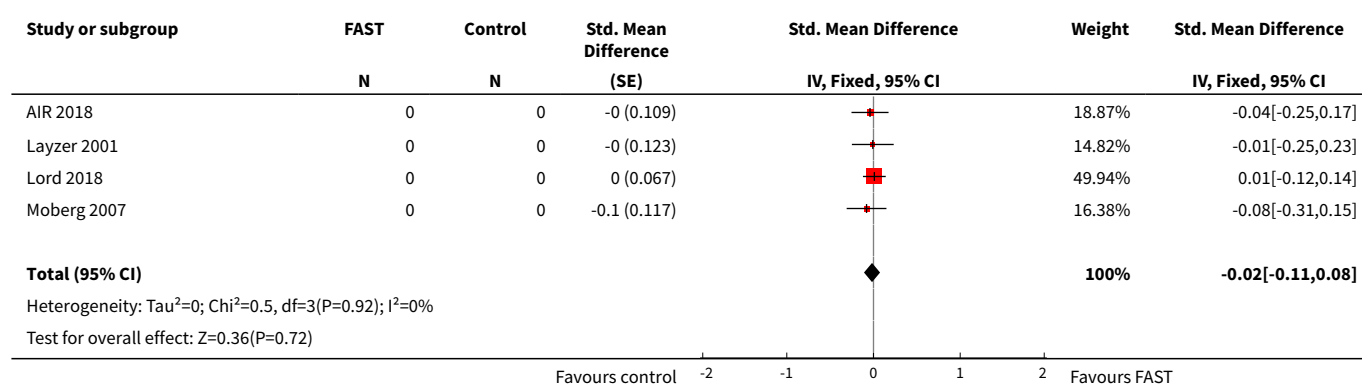
### Comparison 1. Child school performance

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Immediate post-test	2		Std. Mean Difference (Fixed, 95% CI)	-0.06 [-0.34, 0.22]
2 Long-term follow-up	4		Std. Mean Difference (Fixed, 95% CI)	-0.02 [-0.11, 0.08]

#### Analysis 1.1. Comparison 1 Child school performance, Outcome 1 Immediate post-test.



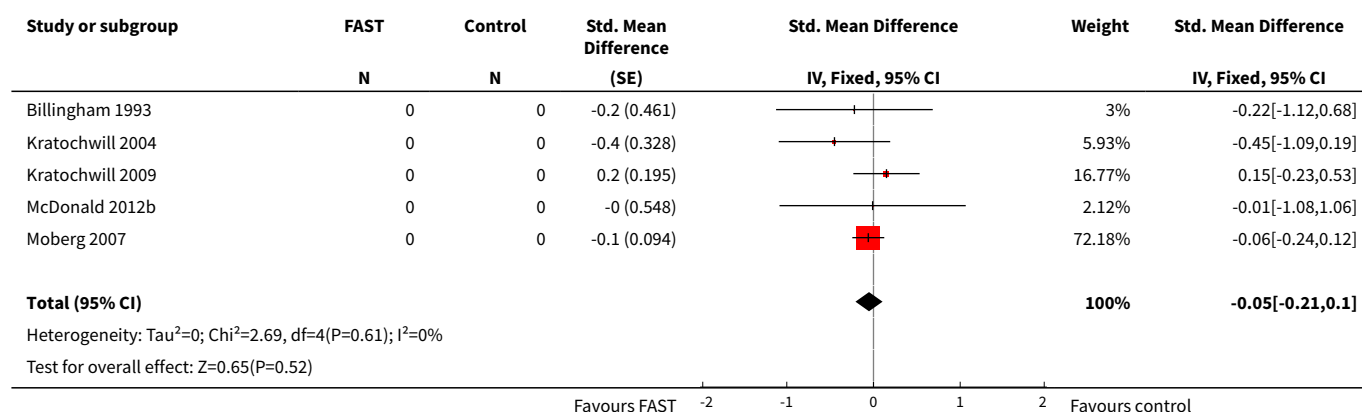
#### Analysis 1.2. Comparison 1 Child school performance, Outcome 2 Long-term follow-up.



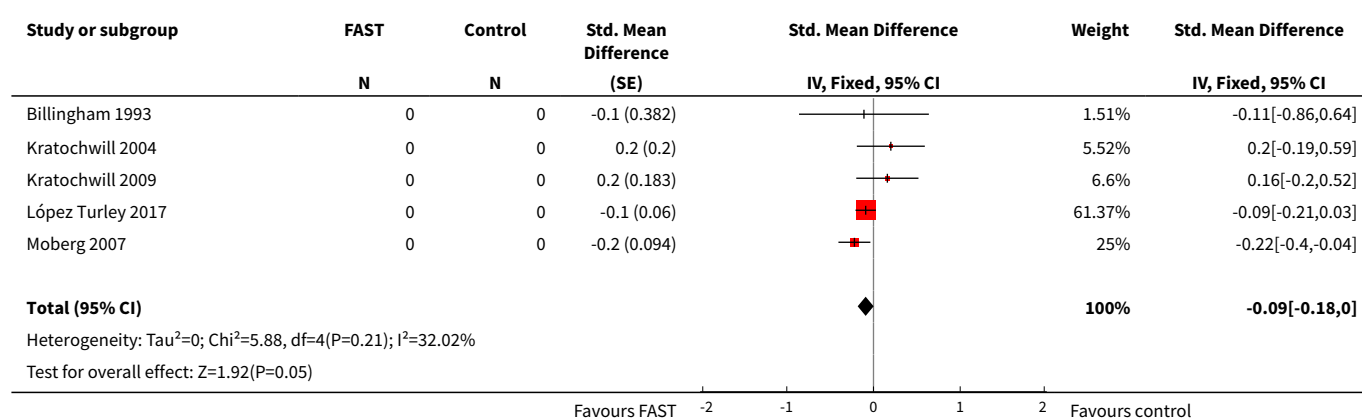
## Comparison 2. Child internalising behaviour

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Immediate post-test, parent report	5		Std. Mean Difference (Fixed, 95% CI)	-0.05 [-0.21, 0.10]
2 Immediate post-test, teacher report	5		Std. Mean Difference (Fixed, 95% CI)	-0.09 [-0.18, 0.00]
3 Long-term follow-up, parent report	4		Std. Mean Difference (Fixed, 95% CI)	0.03 [-0.11, 0.17]
4 Long-term follow-up, teacher report	4		Std. Mean Difference (Fixed, 95% CI)	-0.06 [-0.19, 0.07]

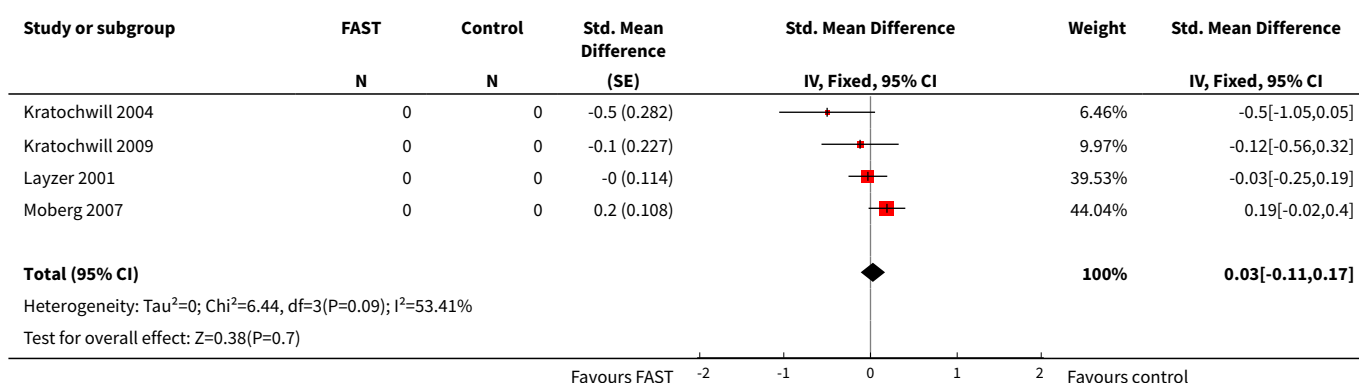
### Analysis 2.1. Comparison 2 Child internalising behaviour, Outcome 1 Immediate post-test, parent report.



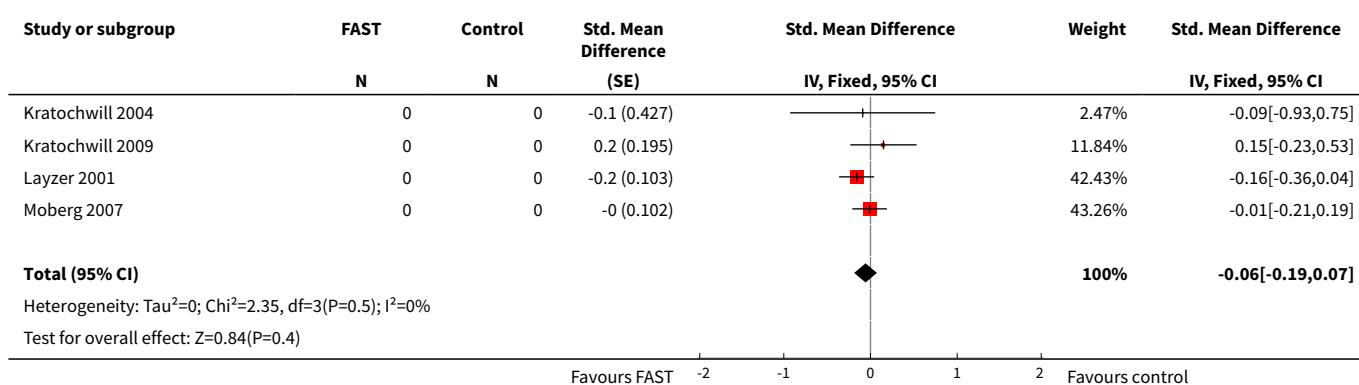
### Analysis 2.2. Comparison 2 Child internalising behaviour, Outcome 2 Immediate post-test, teacher report.



### Analysis 2.3. Comparison 2 Child internalising behaviour, Outcome 3 Long-term follow-up, parent report.



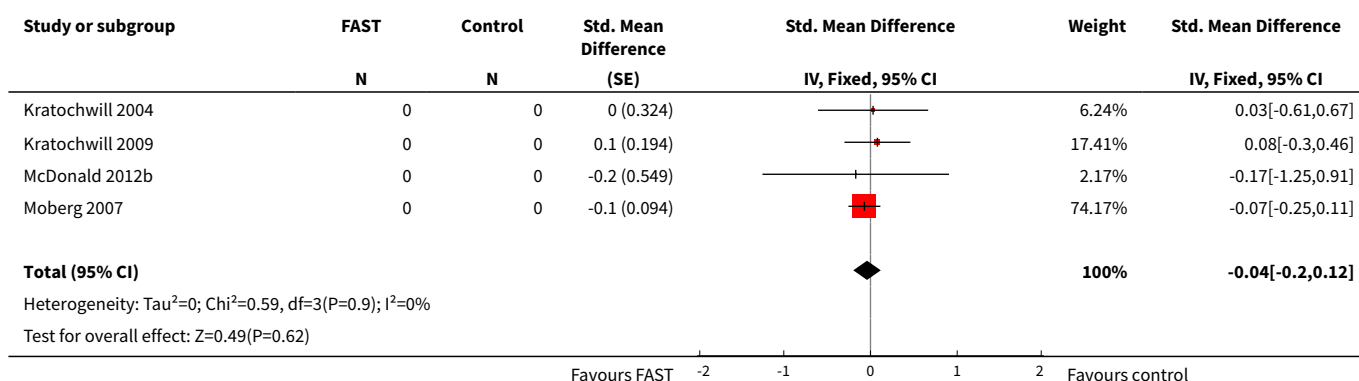
### Analysis 2.4. Comparison 2 Child internalising behaviour, Outcome 4 Long-term follow-up, teacher report.



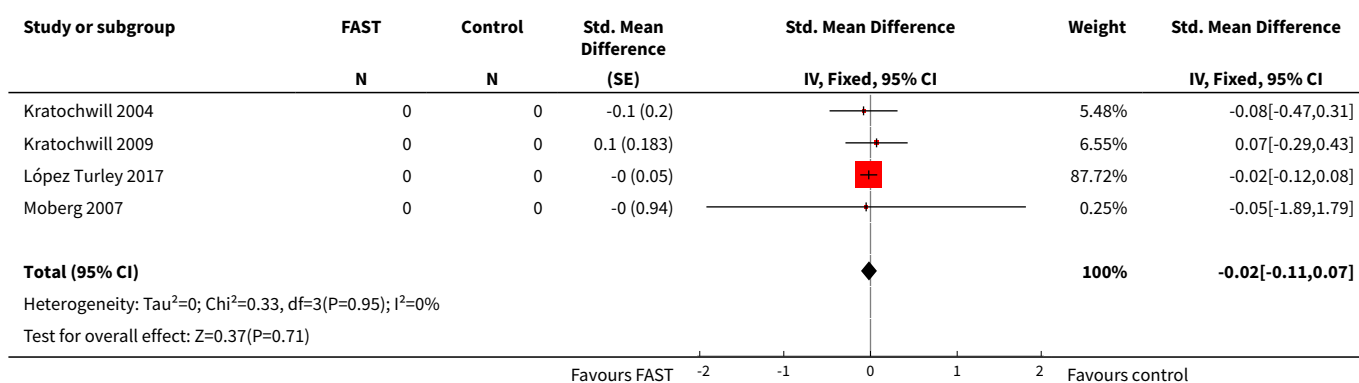
### Comparison 3. Child externalising behaviour

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Immediate post-test, parent report	4		Std. Mean Difference (Fixed, 95% CI)	-0.04 [-0.20, 0.12]
2 Immediate post-test, teacher report	4		Std. Mean Difference (Fixed, 95% CI)	-0.02 [-0.11, 0.07]
3 Long-term follow-up, parent report	4		Std. Mean Difference (Fixed, 95% CI)	-0.19 [-0.32, -0.05]
4 Long-term follow-up, teacher report	4		Std. Mean Difference (Fixed, 95% CI)	-0.10 [-0.24, 0.04]

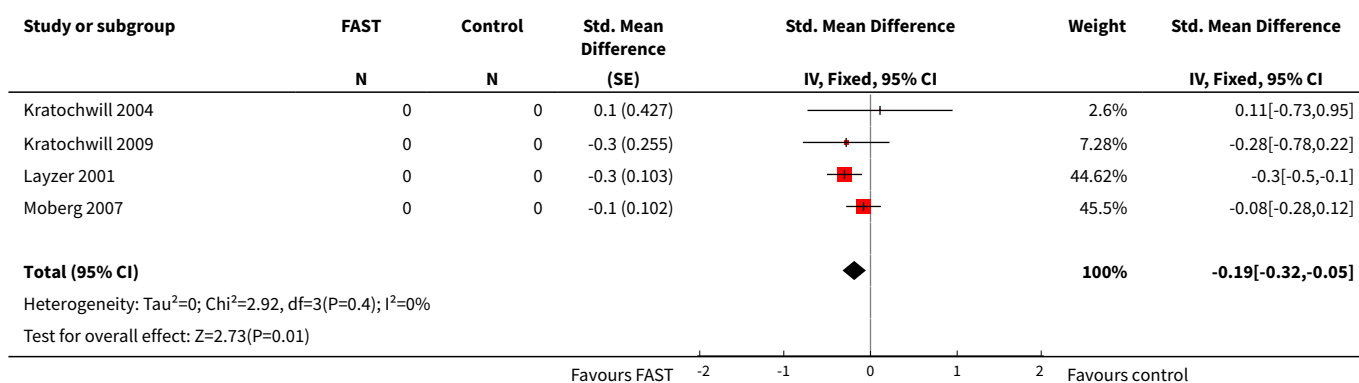
### Analysis 3.1. Comparison 3 Child externalising behaviour, Outcome 1 Immediate post-test, parent report.



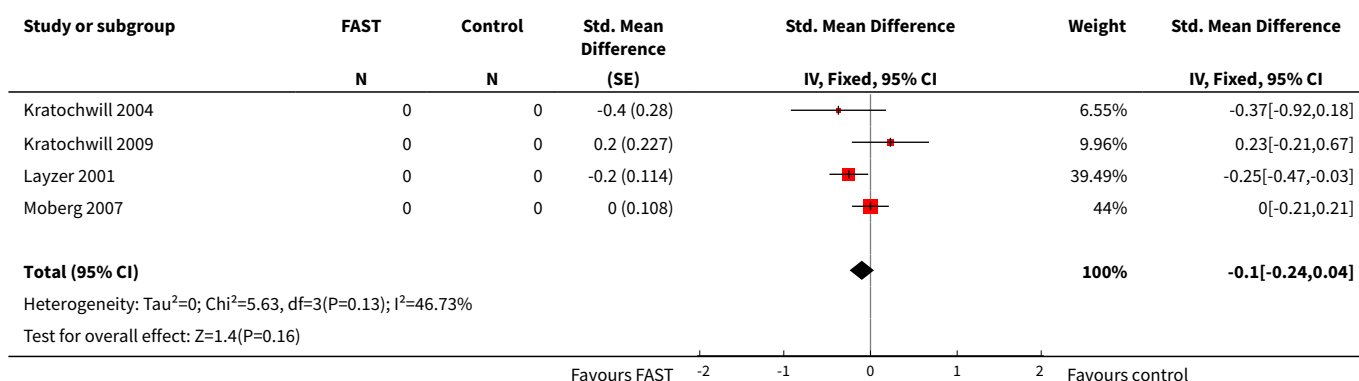
### Analysis 3.2. Comparison 3 Child externalising behaviour, Outcome 2 Immediate post-test, teacher report.



### Analysis 3.3. Comparison 3 Child externalising behaviour, Outcome 3 Long-term follow-up, parent report.



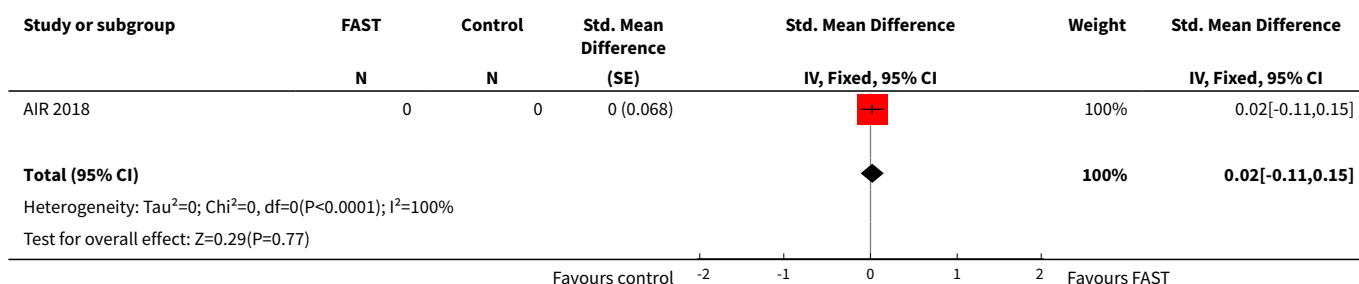
### Analysis 3.4. Comparison 3 Child externalising behaviour, Outcome 4 Long-term follow-up, teacher report.



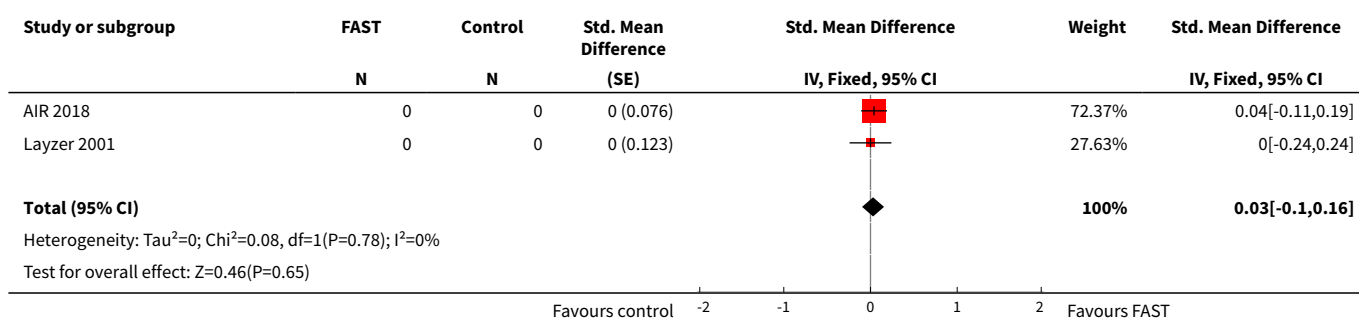
### Comparison 4. Child school attendance

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Short-term follow-up	1		Std. Mean Difference (Fixed, 95% CI)	0.02 [-0.11, 0.15]
2 Long-term follow-up	2		Std. Mean Difference (Fixed, 95% CI)	0.03 [-0.10, 0.16]

#### Analysis 4.1. Comparison 4 Child school attendance, Outcome 1 Short-term follow-up.



#### Analysis 4.2. Comparison 4 Child school attendance, Outcome 2 Long-term follow-up.

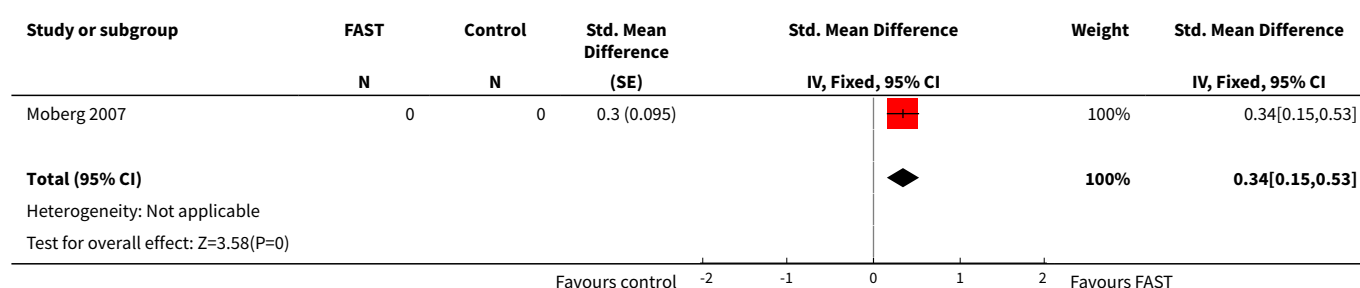




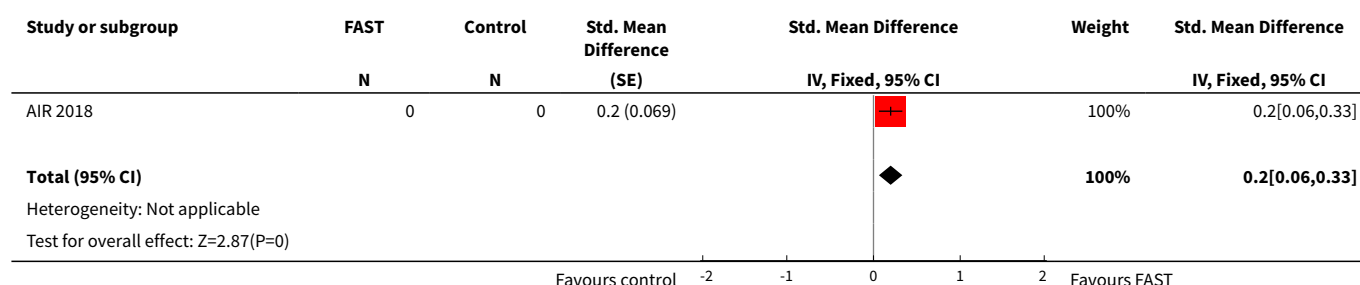
## Comparison 5. Parental engagement with education

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Immediate post-test	1		Std. Mean Difference (Fixed, 95% CI)	0.34 [0.15, 0.53]
2 Short-term follow-up	1		Std. Mean Difference (Fixed, 95% CI)	0.20 [0.06, 0.33]
3 Long-term follow-up	2		Std. Mean Difference (Fixed, 95% CI)	0.03 [-0.07, 0.12]

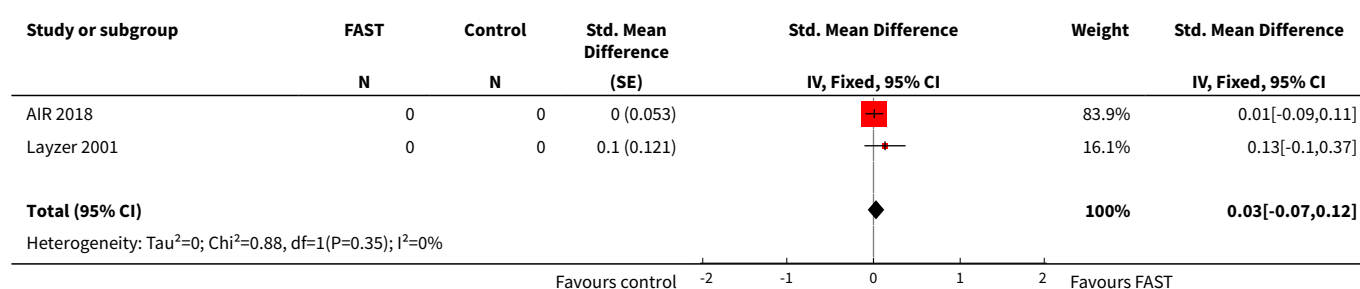
### Analysis 5.1. Comparison 5 Parental engagement with education, Outcome 1 Immediate post-test.

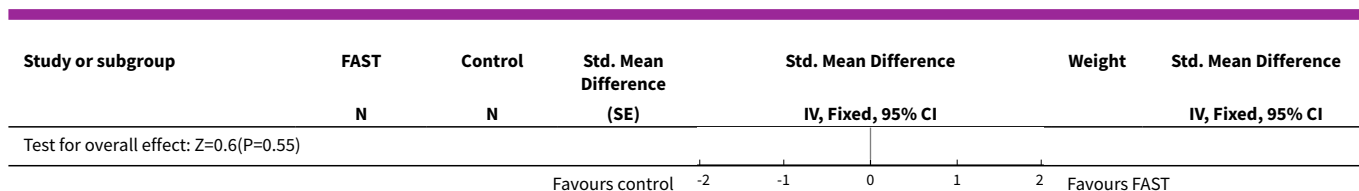


### Analysis 5.2. Comparison 5 Parental engagement with education, Outcome 2 Short-term follow-up.



### Analysis 5.3. Comparison 5 Parental engagement with education, Outcome 3 Long-term follow-up.

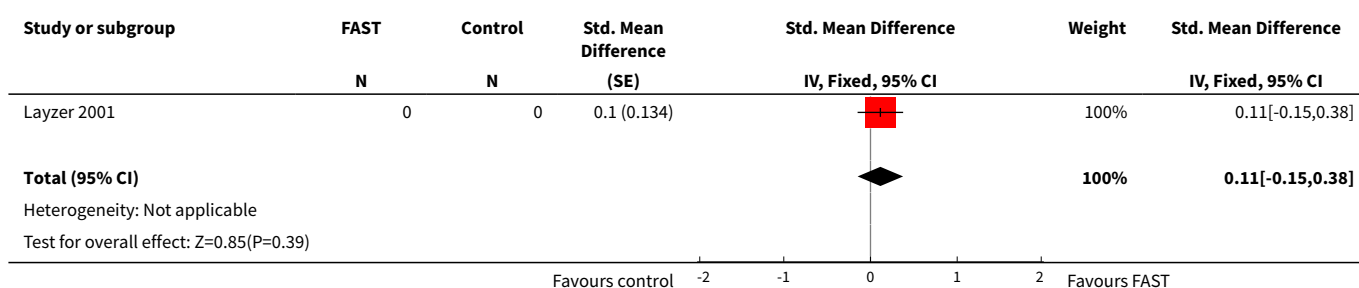




## Comparison 6. Parental involvement in community-based activities

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Long-term follow-up	1		Std. Mean Difference (Fixed, 95% CI)	0.11 [-0.15, 0.38]

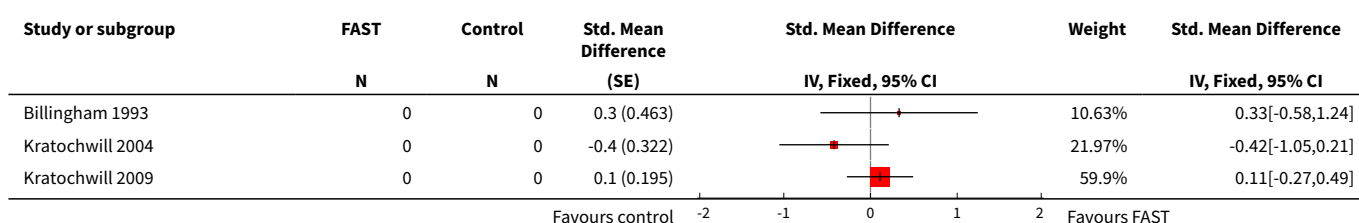
### Analysis 6.1. Comparison 6 Parental involvement in community-based activities, Outcome 1 Long-term follow-up.

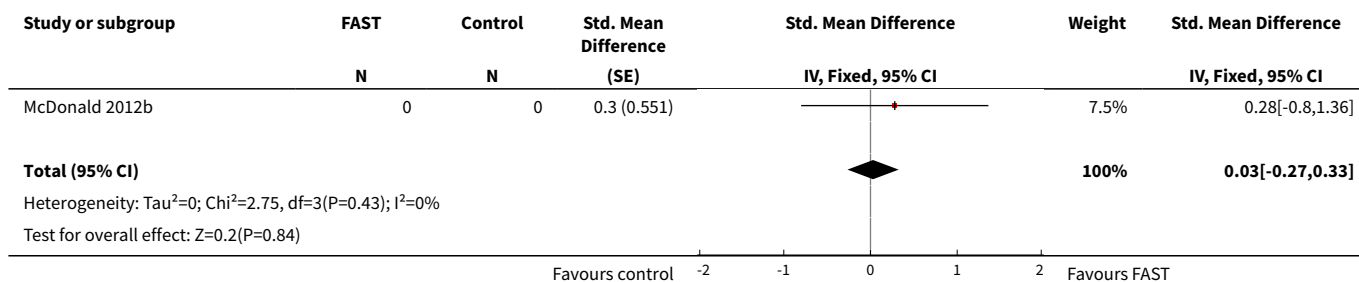


## Comparison 7. Family relationships

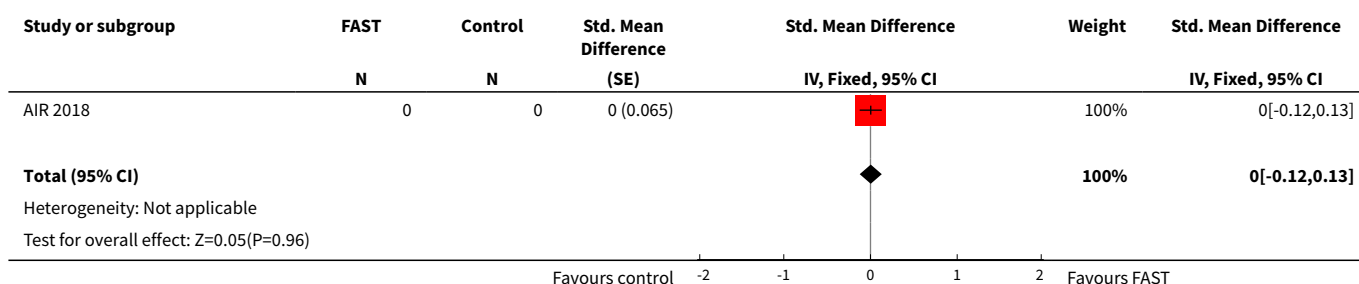
Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Immediate post-test	4		Std. Mean Difference (Fixed, 95% CI)	0.03 [-0.27, 0.33]
2 Short-term follow-up	1		Std. Mean Difference (Fixed, 95% CI)	0.00 [-0.12, 0.13]
3 Long-term follow up	4		Std. Mean Difference (Fixed, 95% CI)	0.08 [-0.03, 0.19]

### Analysis 7.1. Comparison 7 Family relationships, Outcome 1 Immediate post-test.

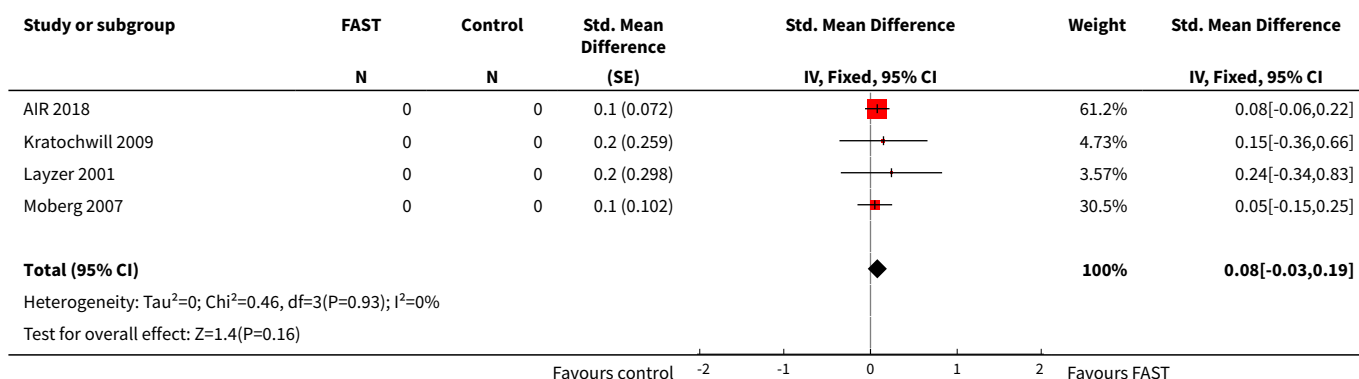




### Analysis 7.2. Comparison 7 Family relationships, Outcome 2 Short-term follow-up.



### Analysis 7.3. Comparison 7 Family relationships, Outcome 3 Long-term follow up.



## ADDITIONAL TABLES

**Table 1. Judgements underpinning 'Risk of bias' assessments**

### Random sequence generation

- Where robust methods of sequence allocation were employed, we recorded the risk of bias as 'low' ([Schultz 2002](#))
- Where non-random or non-systematic approaches were employed, we recorded the risk of bias as 'high'
- Where insufficient detail was provided to make a judgement, we recorded the risk of bias as 'unclear'

### Allocation concealment

**Table 1. Judgements underpinning 'Risk of bias' assessments** *(Continued)*

1. Where robust methods of concealment were employed, and participants and investigators could not determine assignment before allocation, we recorded the risk of bias as 'low'
2. Where the possibility for allocation disclosure and consequent selection bias was present, we recorded the risk of bias as 'high'
3. Where insufficient detail was provided to make a judgement, we recorded the risk of bias as 'unclear'

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**Blinding of participants and personnel**


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1. Where blinding of participants and study personnel was maintained, or where no blinding or incomplete blinding occurred but the review authors judged that the outcome was not likely to have been influenced by the lack of blinding, we recorded the risk of bias as 'low'
  2. Where no or incomplete blinding occurred and could have affected outcomes, or where blinding occurred but there was a likelihood that it could have been broken and the outcome influenced as a result, we recorded the risk of bias as 'high'
  3. Where insufficient detail was provided to make a judgement, we recorded the risk of bias as 'unclear'
- 

**Blinding of outcome assessment**


---

1. Where blinding was robustly applied, there was partial blinding of participants or key personnel, or no blinding took place but the review authors judged that the lack of blinding was unlikely to have affected the measures employed or reported outcomes of the study, we recorded the risk of bias as 'low'
  2. Where incomplete or inefficient blinding occurred, and the measures or outcomes were likely to be affected as a result, we recorded the risk of bias as 'high'
  3. Where insufficient detail was provided to make a judgement, we recorded the risk of bias as 'unclear'
- 

**Incomplete outcome data**


---

1. Where there were no missing data, the reasons for missing data were unlikely to be related to the true outcome, or the effect of missing data was not enough to have a clinically relevant impact, we recorded the risk of bias as 'low'
  2. Where the reason for missing data was likely to have been related to outcomes, or was sufficient to produce a clinically relevant bias, we recorded the risk of bias as 'high'
  3. Where insufficient detail was provided to make a judgement, we recorded the risk of bias as 'unclear'
- 

**Selective outcome reporting**


---

1. Where outcomes were reported in accordance with the protocol, or all expected outcomes were presented, we recorded the risk of bias as 'low'
  2. Where there was some variance in reporting outcomes from those specified in the protocol, reporting was incomplete, or the study failed to include results for a key outcome, we recorded the risk of bias as 'high'
  3. Where insufficient detail was provided to make a judgement, we recorded the risk of bias as 'unclear'
- 

**Table 2. Methods for use in future updates of this review**

Issue	Method
<b>Measures of treatment effect</b>	<b>Continuous data</b>  Where necessary, we will compute effect measures from P values, t statistics, analysis of variance (ANOVA) tables, or other statistics
<b>Unit of analysis issues</b>	<b>Cluster-randomised trials</b>  Where clustering has been appropriately accounted for within the analysis of the original study data, clustered data can be used in a meta-analysis. However, a 'unit of analysis' error occurs when data from cluster-randomised trials have been analysed as though the unit of allocation has been the individual rather than the cluster. In these circumstances, corrections are required to produce accurate effect size estimates (Section 16.3.4; Higgins 2011c). To calculate the design effect, we

**Table 2. Methods for use in future updates of this review** (Continued)

	<p>need a measure of the relative variation both within and between clusters. This is known as the intra-class correlation coefficient (ICC). Where the ICC from the original trial is not available, we will use external estimates from similar studies to calculate the design effect. If there are no reported estimates in the literature, we will perform a sensitivity analysis using low (0.01), medium (0.05), and high (0.10) values for ICC. However, as the design effect must be rounded up for entry into RevMan 5 (<a href="#">Review Manager 2014</a>), this approach may be unsuitable for small studies, and we may need to employ an alternative approach that multiplies the standard errors (SEs) of the effect size by the square root of the design effect. In either case, where we include cluster-randomised trials in the meta-analysis, we will clearly identify them and will explain the method of calculating effect size estimates and their standard errors. In these circumstances, we will employ a sensitivity analysis to test the robustness of any conclusions deduced from these methods (see Sensitivity Analysis below)</p>
	<p><b>Multiple treatment arms</b></p> <p>We do not anticipate finding studies with multiple treatment groups. However, should we identify such studies, we will first combine all eligible intervention arms and compare these with all control arms, making a single, pair-wise comparison. If such a strategy seems likely to prevent the investigation of important sources of heterogeneity, we will keep intervention arms separate and will compare each with a common control group, dividing the sample size of the latter proportionately across each comparison, thereby preventing double counting of individuals (Section 16.5.5; <a href="#">Higgins 2011c</a>)</p>
<b>Dealing with missing data</b>	<p>Where we are certain that missing data are 'missing at random' and unlikely to be related to the characteristics of the participants or study design, we will analyse the available data while ignoring the missing data (<a href="#">Higgins 2011c</a>). Conversely, where there is no reason to believe that data are missing at random - that is, as a result of publication or selective reporting bias - we will work with a statistician to select replacement values using imputed mean values or multiple imputation methods</p>
<b>Assessment of reporting biases</b>	<p>Where 10 or more studies provide data on a particular outcome, we will draw funnel plots (estimated differences in treatment effects against their standard error). Symmetrical funnel plots are associated with low levels of bias. Asymmetrical funnel plots may reflect publication bias, but they can also reflect real relationships between trial size and effect size, such as when larger trials have lower compliance, and compliance is positively related to effect size. If we have reason to think that this is happening, we will look for a possible explanation in clinical variation across studies</p> <p>To test directly for publication bias, we will conduct a sensitivity analysis (see Sensitivity Analysis below) to compare results from published data vs unpublished data and data from other sources</p>
<b>Data synthesis</b>	<p>In future updates of this review, should we encounter serious funnel plot asymmetry, we will assume that neither the fixed-effect nor the random-effects models are appropriate and will present the results of both. Where both indicate the presence or absence of an effect, we will assume that we can have some confidence in the results. Where they disagree, we will report this</p> <p>In future, if some included studies report an outcome using dichotomous outcome measures and others use continuous measures, we will convert results from the former, from an OR to an SMD, as long as there is reason to assume that the underlying continuous measure approximates a normal or logistical distribution. Where this is not the case, we will conduct separate analyses</p>
<b>Subgroup analysis and investigation of heterogeneity</b>	<p>As the overuse of subgroup analysis is problematic (<a href="#">Deeks 2011</a>), we will use subgroup analyses only to determine a small number of effect modifiers</p> <p>We will conduct the following 4 subgroup analyses.</p>

**Table 2. Methods for use in future updates of this review** *(Continued)*

1. Differences in treatment effect between each of the FAST variants (correlated to ages of child participants), namely:
  - a. Baby FAST;
  - b. Pre-K FAST;
  - c. Kids' FAST;
  - d. Middle School FAST; and
  - e. Teen FAST.

N.B. Since the protocol was published ([Macdonald 2017](#)), the various FAST programmes have been renamed as Baby FAST, Elementary Level, Middle School Level, and High School Level FAST.

1. Programmes evaluated by teams independent of the programme developer vs those involving the programme developer, as there is evidence to suggest that effect sizes reported in studies involving the programme developer are larger than those in studies conducted entirely independently.
2. Location, exploring the possible impact of FAST in countries at differing stages of economic development.
3. Ethnicity: because [Moberg 2007](#) noted that Latino families are 12% more likely to graduate from FAST, and more than twice as likely to attend FASTWORKS than African Americans, we will consider a subgroup analysis of the ethnicity of participants or cultural adaptation of the programme (or both) and implementation of FASTWORKS

As no family size effects are noted in [Crozier 2010](#), [McDonald 2009b](#), and [McDonald 2010](#), despite reports of differing average family sizes, we will not include family size in the subgroup analysis.

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**Sensitivity analysis**

We will use sensitivity analyses to explore the impact of studies at high risk of bias on the robustness of review results, restricting the analyses to (1) studies or outcomes with low risk of assessment bias, (2) studies with low risk of attrition bias, and (3) studies with low risk of reporting bias. In addition:

1. where RCTs and quasi-RCTs are included in a meta-analysis, we will explore the impact of removing the quasi-RCT studies;
  2. where 1 or 2 studies appear to be 'outliers' (have results very different from the remainder), we will examine the impact of excluding these from the meta-analysis;
  3. where the results of a meta-analysis appear to be heavily dependent on one particular trial, we will repeat the analysis while excluding this trial (which may be the largest, or the earliest); and
  4. we may examine the effects of different ICCs for cluster-randomised trials.
- 

FAST: Families and Schools Together.

FASTWORKS: Families and Schools Together, Working, Organising, Relaxing, Knowing, Sharing.

OR: odds ratio.

RCT: randomised controlled trial.

SMD: standardised mean difference.

## APPENDICES

### Appendix 1. Search strategies

#### Cochrane Central Register of Controlled Trials (CENTRAL), in the Cochrane Library

- 1 Professional-Family Relations
- 2 parent-child relations
- 3 1 or 2
- 4 Schools
- 5 3 and 4
- 6 (FAST\* not food\*)
- 7 5 and 6
- 8 (Famil\* near/3 School\* Together)
- 9 ((multifamil\* or multi-famil\*) near/5 (afterschool\* or after-school\*))

**Families and Schools Together (FAST) for improving outcomes for children and their families (Review)**

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10 (parent\* near/3 School\* near/3 partner\*)  
 11 (parent\* near/3 teacher\* near/3 partner\*)  
 12 (famil\* near/3 teacher\* near/3 partner\*)  
 13 (famil\* near/3 School\* near/3 partner\*)  
 14 (((Baby or Pre-K or Kids or middle school or Teen\*) near/3 FAST) not food\*)  
 15 (FASTWorks or FAST-Works)  
 16 or/8-15  
 17 7 or 16

#### MEDLINE Ovid

1 Professional-Family Relations/  
 2 parent-child relations/  
 3 1 or 2  
 4 Schools/  
 5 3 and 4  
 6 (FAST\$ not food\$).tw,kw.  
 7 5 and 6  
 8 (Famil\$ adj3 School\$ Together).tw,kw.  
 9 ((multifamil\$ or multi-famil\$) adj5 (afterschool\$ or after-school\$)).tw,kw.  
 10 (parent\$ adj3 School\$ adj3 partner\$).tw,kw.  
 11 (parent\$ adj3 teacher\$ adj3 partner\$).tw,kw.  
 12 (famil\$ adj3 teacher\$ adj3 partner\$).tw,kw.  
 13 (famil\$ adj3 School\$ adj3 partner\$).tw,kw.  
 14 (((Baby or Pre-K or Kids or middle school or Teen\$) adj3 FAST) not food\$).tw,kw.  
 15 (FASTWorks or FAST-Works).tw,kw.  
 16 or/8-15  
 17 7 or 16

#### Embase Elsevier

1 'professional-family relations'  
 2 parent-child relations'  
 3 #1 or #2  
 4 schools  
 5 #3 and #4  
 6 fast\* not food\*  
 7 #5 and #6  
 8 famil\* near/3 school\* and together  
 9 (multifamil\* or 'multi-famil\*') near/5 (afterschool\* or 'after-school\*')  
 10 parent\* near/3 school\* near/3 partner\*  
 11 parent\* near/3 teacher\* near/3 partner\*  
 12 famil\* near/3 teacher\* near/3 partner\*  
 13 famil\* near/3 school\* near/3 partner\*  
 14 (baby or 'pre-k' or kids or 'middle school' or teen\*) near/3 fast\* not food\*  
 15 fastworks or 'fast-works'  
 16 #7 or #8 or #9 or #10 or #11 or #12 or #13 or #14 or #1

#### PsycINFO EBSCOhost

1 Professional-Family Relations/  
 2 parent-child relations/  
 3 1 or 2  
 4 Schools/  
 5 3 and 4  
 6 (FAST\* not food\*)  
 7 5 and 6  
 8 (Famil\* n3 School\* Together)  
 9 ((multifamil\* or multi-famil\*) n5 (afterschool\* or after-school\*))  
 10 (parent\* n3 School\* n3 partner\*)  
 11 (parent\* n3 teacher\* n3 partner\*)  
 12 (famil\* n3 teacher\* n3 partner\*)  
 13 (famil\* n3 School\* n3 partner\*)  
 14 (((Baby or Pre-K or Kids or middle school or Teen\*) n3 FAST) not food\*)

15 (FASTWorks or FAST-Works)

16 or/8-15

17 7 or 16

#### **ERIC (Education Resources Information Center) EBSCOhost**

S1 DE 'Partnerships in Education' or DE 'Student School Relationship' or DE 'After School Programs' or DE 'After School Education' or DE 'Enrichment Activities' or DE 'Extracurricular Activities'

S2 DE 'Parents' or DE 'Fathers' or DE 'Grandparents' or DE 'Mothers' or DE 'Family (Sociological Unit)' or DE 'Family Programs' or DE 'Family Involvement'

S3 S1 and S2

S4 Family School Relationship

S5 S3 or S4

S6 FAST\* not food\*

S7 S5 and S6

S8 Famili\* n3 School\* Together

S9 (multifamil\* or multi-famil\*) n5 (afterschool\* or after-school\*)

S10 (parent\* n3 school\* n3 partner\*)

S11 (parent\* n3 teacher\* n3 partner\*)

S12 (famil\* n3 teacher\* n3 partner\*)

S13 (famil\* n3 school\* n3 partner\*)

S14 (Baby or Pre-K or Kids or middle school or Teen\*) near/3 fast\* not food\*

S15 FASTWORKS or FAST-works

S16 S7 or S8 or S9 or S10 or S11 or S12 or S13 or S14 or S15

#### **British Education Index (BEI) EBSCOhost**

S1 DE "PARENT participation in education" OR DE "MATHEMATICS -- Study & teaching -- Parent participation" OR DE "PARENT participation in children's reading" OR DE "PARENT participation in early childhood education" OR DE "PARENT participation in elementary education" OR DE "PARENT participation in health education" OR DE "PARENT participation in higher education" OR DE "PARENT participation in middle school education" OR DE "PARENT participation in music education" OR DE "PARENT participation in school administration" OR DE "PARENT participation in secondary education" OR DE "PARENT participation in special education" OR DE "PARENT participation in vocational education" OR DE "SCIENCE -- Study & teaching -- Parent participation"

S2 ((DE "PARENT & child") OR (DE "PARENT-student relationships")) OR (DE "PARENT-teacher cooperation" OR DE "PARENT-teacher relationships") OR DE "FAMILY-school relationships"

S3 S1 OR S2

S4 (FAST\*)

S5 S3 AND S4

S7 ( School\* N3 together\*)

S8 ((multifamil\* or multi-famil\*) N5 (afterschool\* or after-school\*))

S9 (parent\* N3 School\* N3 partner\*)

S10 (parent\* N3 teacher\* N3 partner\*)

S11 (famil\* N3 teacher\* N3 partner\*)

S12 (famil\* N5 partnership\*)

S13 ((Baby or Pre-K or Kids or "middle school" or Teen\*) N3 FAST)

S14 (FASTWorks or FAST-Works)

S15 S5 OR S6 OR S7 OR S8 OR S9 OR S10 OR S11 OR S12 OR S13

#### **ProQuest Education Database**

((("families and schools together") OR (FASTworks OR "Fast-works")) OR (TI((Baby OR Pre-K OR Kids OR "middle school" OR Teen\*) NEAR/1" FAST)) OR AB((Baby OR Pre-K OR Kids OR "middle school" OR Teen\*) NEAR/1" FAST)) OR (AB(("parent\* teacher\* partner\*")) OR TI(("parent\* teacher\* partner\*")) OR (TI("famil\* school\* partner\*")) OR AB ("famil\* school\* partner\*")) OR (TI("parent\* school\* partner\*")) OR AB ("parent\* school\* partner\*")) OR (TI("famil\* teacher\* partner\*")) OR AB ("famil\* teacher\* partner\*")) OR (TI("parent\* teacher\* partner\*")) OR AB ("parent\* teacher\* partner\*"))

#### **Education Abstracts (HW Wilson) EBSCOhost**

S1 DE "Parent participation in education" OR DE "Mathematics -- Study & teaching -- Parent participation" OR DE "Parent participation in children's reading" OR DE "Parent participation in early childhood education" OR DE "Parent participation in elementary education" OR DE "Parent participation in middle school education" OR DE "Parent participation in music education" OR DE "Parent participation in secondary education" OR DE "Parent participation in special education" OR DE "Science -- Study & teaching -- Parent participation"

S2 DE "Parent-student relationships" OR DE "Parent-teacher cooperation" OR DE "Parent-teacher relationships"

S3 S1 OR S2

S4 DE "Partnerships in Education" OR DE "Student School Relationship" OR DE "After School Programs" OR DE "After School Education" OR DE "Enrichment Activities" OR DE "Extracurricular Activities"

S5 DE "Parents" OR DE "Fathers" OR DE "Grandparents" OR DE "Mothers" OR DE "Family (Sociological Unit)" OR DE "Family Programs" or DE "Family Involvement")

S6 S4 AND S5

S7 DE "Family-school relationships" OR DE "Parent-school relationships" OR DE "Community & school"

S8 S3 or S6 OR S7

S9 TI (FAST\* NOT food\*) OR AB (FAST\* NOT food\*)

S10 S8 AND S9 8

S11 TI (Famil\* N3 School\* Together) OR AB (Famil\* N3 School\* Together)

S12 TI ((multifamil\* or multi-famil\*) N5 (afterschool\* or after-school\*)) OR AB ((multifamil\* or multi-famil\*) N5 (afterschool\* or after-school\*))

S13 TI (parent\* N3 School\* N3 partner\*) OR AB (parent\* N3 School\* N3 partner\*)

S14 TI (parent\* N3 teacher\* N3 partner\*) OR AB (parent\* N3 teacher\* N3 partner\*)

S15 TI (famil\* N3 School\* N3 partner\*) OR AB (famil\* N3 School\* N3 partner\*)

S16 TI (Famil\* N3 teacher\* N3 partner\*) OR AB (famil\* N3 teacher\* N3 partner\*)

S17 (((Baby or Pre-K or Kids or middle school or Teen\*) N3 FAST\*) not food\*)

S18 FASTWORKS or FAST-WORKS

S19 S10 OR S11 OR S12 OR S13 OR S14 OR S15 OR S16 OR S17 OR S18

### Social Science Citation Index Web of Science

1 TOPIC: (Professional-Family Relations/) Indexes=SSCI Timespan=All years

2 TOPIC: (parent-child relations/) Indexes=SSCI Timespan=All years

3 #2 or #1 Indexes=SSCI Timespan=All years

4 TOPIC: (schools) Indexes=SSCI Timespan=All years

5 #4 and #3 Indexes=SSCI Timespan=All years

6 TOPIC: (fast\* not food\*) Indexes=SSCI Timespan=All years

7 #6 and #5 Indexes=SSCI Timespan=All years

8 TOPIC: (Famil\* near/3 School\* Together) Indexes=SSCI Timespan=All years

9 TOPIC: (multifamil\* or multi-famil\*) near/5 (afterschool\* or after-school\*) Indexes=SSCI Timespan=All years

10 TOPIC: (parent\* near/3 school\* near/3 partner\*) Indexes=SSCI Timespan=All years

11 TOPIC: (parent\* near/3 teacher\* near/3 partner\*) Indexes=SSCI Timespan=All years

12 TOPIC: (famil\* near/3 teacher\* near/3 partner\*) Indexes=SSCI Timespan=All years

13 TOPIC: (famil\* near/3 school\* near/3 partner\*) Indexes=SSCI Timespan=All years

14 TOPIC: ((Baby near/3 fast) not food\*) Indexes=SSCI Timespan=All years

15 TOPIC: ((Pre-K near/3 fast) not food\*) Indexes=SSCI Timespan=All years

16 TOPIC: ((Kids near/3 fast) not food\*) Indexes=SSCI Timespan=All years

17 TOPIC: ((middle school near/3 fast) not food\*) Indexes=SSCI Timespan=All years

18 TOPIC: ((Teen\* near/3 fast) not food\*) Indexes=SSCI Timespan=All years

19 TOPIC: FASTWORKS or FAST-works Indexes=SSCI Timespan=All years

20 #19 or #18 or #17 or #16 or #15 or #14 or #13 or #12 or #11 or #10 or #9 or #8 or #7 Indexes=SSCI Timespan=All years

### Conference Proceedings Citation Index - Social Science and Humanities Web of Science

# 18 #17 OR #16 OR #15 OR #14 OR #13 OR #12 OR #11 OR #10 OR #9 OR #8 OR #6 OR #5 OR #4 OR #3 OR #2 OR #1

Indexes=CPCI-SSH Timespan=All years

# 17 TS=(school\* near/3 "family relation\*")

Indexes=CPCI-SSH Timespan=All years

# 16 TS=(teacher\* near/3 "family relation\*")

Indexes=CPCI-SSH Timespan=All years

# 15 TS=(professional near/3 "Family relation\*")

Indexes=CPCI-SSH Timespan=All years

# 14 TS=((parent near/3 "child relation\*") AND school\*)

Indexes=CPCI-SSH Timespan=All years

# 13 TS=(FASTworks OR FAST-works)

Indexes=CPCI-SSH Timespan=All years

# 12 TS=((teen\* Near/3 FAST) NOT food\*)

Indexes=CPCI-SSH Timespan=All years

# 11 TS=((("middle school" Near/3 FAST) NOT food\*))

Indexes=CPCI-SSH Timespan=All years

# 10 TS=((Kids Near/3 FAST) NOT food\*)

Indexes=CPCI-SSH Timespan=All years

# 9 TS=((Pre-K Near/3 FAST) NOT food\*))  
Indexes=CPCI-SSH Timespan=All years  
# 8 TS=((Baby Near/3 FAST) NOT food\*))  
Indexes=CPCI-SSH Timespan=All years  
# 7 TS=(Famil\* NEAR/3 teacher\* NEAR/3 partner\*)  
Indexes=CPCI-SSH Timespan=All years  
# 6 TS=(famil\* NEAR/3 school\* NEAR/3 partner\*)  
Indexes=CPCI-SSH Timespan=All years  
# 5 TS=(parent\* NEAR/3 school\* NEAR/3 partner\*)  
Indexes=CPCI-SSH Timespan=All years  
# 4 TS=(Parent\* NEAR/3 teacher\* NEAR/3 partner\*)  
Indexes=CPCI-SSH Timespan=All years  
# 3 TS=((multifamil\* or multi-famil\*) Near/5 (afterschool\* or after-school\*))  
Indexes=CPCI-SSH Timespan=All years  
# 2 TS=((("FAST" Near/3 school\*) NOT food\*))  
Indexes=CPCI-SSH Timespan=All years  
# 1 TS=((("FAST" Near/3 famil\*) NOT food\*))  
Indexes=CPCI-SSH Timespan=All years

#### **EPPI Centre Database of Education Research ([eppi.ioe.ac.uk/webdatabases/Intro.aspx?ID=6](http://eppi.ioe.ac.uk/webdatabases/Intro.aspx?ID=6))**

EPPI Centre: (FAST or FASTWORKS or FAST-works or (families and schools and together)

#### **Campbell Library of Systematic Reviews ([www.campbellcollaboration.org/library.html](http://www.campbellcollaboration.org/library.html))**

(FAST or (families and schools))

#### **Cochrane Database of Systematic Reviews (CDSR), part of the Cochrane Library**

1 Professional-Family Relations  
2 parent-child relations  
3 #1 or #2  
4 Schools  
5 #3 and #4  
6 (FAST\* not food\*)  
7 #5 and #6  
8 (Famil\* near/3 School\* Together)  
9 ((multifamil\* or multi-famil\*) near/5 (afterschool\* or after-school\*))  
10 (parent\* near/3 School\* near/3 partner\*)  
11 (parent\* near/3 teacher\* near/3 partner\*)  
12 (famil\* near/3 teacher\* near/3 partner\*)  
13 (famil\* near/3 School\* near/3 partner\*)  
14 (((Baby or Pre-K or Kids or middle school or Teen\*) near/3 FAST) not food\*)  
15 (FASTWorks or FAST-Works)  
16 #7 or #8 or #9 or #10 or #11 or #12 or #13 or #14 or #15

#### **Database of Abstracts of Reviews of Effectiveness (DARE; [www.crd.york.ac.uk/CRDWeb/](http://www.crd.york.ac.uk/CRDWeb/))**

families and schools and together

#### **Epistemonikos ([www.epistemonikos.org](http://www.epistemonikos.org))**

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**UK Clinical Trials Gateway ([www.ukctg.nihr.ac.uk/clinical-trials](http://www.ukctg.nihr.ac.uk/clinical-trials))**

families or FAST, limiting to children (3-12) or adolescents (13-17) and to recruiting and completed trials.

**ClinicalTrials.gov (<https://clinicaltrials.gov/>)**

(families and schools and together; intervention studies) or (families and schools and FAST; intervention studies; child)

**WHO International Clinical Trials Registry Platform (ICTRP; [www.who.int/ictcp/en](http://www.who.int/ictcp/en))**

(families and schools) or (parent and school and partner\*)

**CONTRIBUTIONS OF AUTHORS**

Geraldine Macdonald (GM) is responsible for conceiving, designing, and co-ordinating the review; providing general advice; and contributing to development of the protocol and the review. She also secured funding for the review.

Jeffrey C Valentine (JV) contributed to development of the protocol.

JV, Allison Fowler, Steve Leach, and Diana Stojda screened, retrieved, and coded studies (including conducting the 'Risk of bias' assessments).

JV and GM wrote the narrative sections of the review.

JV is the guarantor of the review.

**DECLARATIONS OF INTEREST**

Jeffrey C Valentine is a Statistician with the Cochrane Developmental Psychosocial and Learning Problems (CDPLP) Review Group.

Geraldine Macdonald is the Co-ordinating Editor of CDPLP. She did not take an active role in the editorial process for this review.

Stephen M Leach - none known.

Allison P Fowler - none known.

Diana Stojda - none known.

**SOURCES OF SUPPORT****Internal sources**

- University of Bristol, UK.

Salary for Geraldine Macdonald

- University of Louisville, USA.

Salary for Jeff Valentine

**External sources**

- HSC R&D Division Public Health Agency, UK.

Provided funding to support Clive Robinson in the development of this protocol.

**DIFFERENCES BETWEEN PROTOCOL AND REVIEW**

1. **Authorship:** Nuala Livingstone assisted with development of the protocol but not the review.

2. [Types of outcome measures](#)

- a. The protocol did not articulate a rule for handling situations in which more than one informant contributed to an outcome ([Macdonald 2017](#)). This situation arose in five studies that provided parent and teacher reports of student behaviour. Our options were to (i) drop one informant (either randomly or using an ad hoc rule), (ii) synthesise the multiple informant outcomes separately but create an ad hoc rule about which of these to present in the 'Summary of findings' table, (iii) create for each study a synthetic effect size that represents the mean of the multiple informants, and (iv) use robust variance estimation to account for the dependent nature of the outcomes based on multiple informants. We did not seriously entertain option (i) and ultimately chose option (ii). Based on our belief that parents observe children's behaviour in a wider variety of contexts, we present results for parent reports of behaviour in the 'Summary of findings' table. The results for teacher reports of behaviour are available in [Analysis 2.2](#), [Analysis 2.4](#), [Analysis](#)

3.2, and Analysis 3.4. We did not analytically explore the effects of choosing option (iii) or (iv), but the effect sizes are similar across informants, suggesting that our choice of analytical strategy is unlikely to have affected our conclusions.

### 3. Electronic searches

- a. The protocol specified that we would run the PsycINFO database search on the Ovid platform (Macdonald 2017). However, due to a subscription change at the University of Louisville's library, we ran the original and top-up searches on the EBSCO platform.

### 4. Searching other resources

- a. We originally planned on contacting FAST practitioners in an effort to identify previously unidentified studies (Macdonald 2017). We did not do this because FAST practitioners are extremely difficult to identify.

### 5. Selection of studies

- a. We originally planned on retrieving the full text of any citation deemed potentially relevant by at least one review author (Macdonald 2017), but we ultimately retrieved the full text of any citation that was identified by at least two review authors as potentially relevant (the two review authors worked independently, and a third review author (usually JV) reconciled disagreements).

### 6. Unit of analysis issues

- a. Because there were so few statistically significant results, we did not implement the cluster correction as described in the protocol - Macdonald 2017 - and in Table 2, as doing so would have led to larger reported probability values and CIs but would not have changed our interpretation of the results in any way.

### 7. Dealing with missing data

- a. We originally planned on using imputation or multiple imputation for missing data when we believed that missingness could be ignorable (i.e. when data were missing at random) (Macdonald 2017); see also Table 2. Knox 2011 did not provide standard deviations for effect size calculations for any eligible outcomes, and Kratochwill 2004 did not provide standard deviations for the long-term follow-up of academic achievement. In both cases, the missingness is plausibly random. However, no other studies in our review used the same measures as those employed in these studies, and therefore we could not 'borrow' standard deviations from other studies to impute the missing data.

### 8. Grade criteria

- a. In line with current guidance, we amended the GRADE criteria to refer to 'certainty' rather than 'quality' of evidence.

### 9. 'Summary of findings' tables

- a. The protocol did articulate a rule for handling effect sizes presented at different points in time but did not suggest which of these should be presented in the 'Summary of findings' table (Macdonald 2017). We chose to present the effect size that was assessed at follow-up in the 'Summary of findings' table.

### 10. Grouping of different endpoints

- a. We anticipated that studies would assess outcomes at different points after completion of the intervention. We originally planned to group studies by short term (up to one year of follow-up), medium term (between one and two years of follow-up), and long term (over two years of follow-up). After coding the studies but before we conducted any data analysis, we revised these categories to (i) immediately after the end of the intervention (zero to two months), (ii) short-term follow-up (three to nine months), and (iii) long-term follow-up (10+ months after completion of the intervention), largely due to the desire to separate outcomes that were measured essentially immediately after the intervention from those that were measured at more traditional follow-up periods.